# PROCEEDINGS

OF

# THE AMERICAN ASSOCIATION

FOR THE

# ADVANCEMENT OF SCIENCE

FOR THE FORTY-SECOND MEETING.

HELD AT

MADISON, WISCONSIN.

AUGUST, 1893.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
1894.

EDITED BY

FREDERIC W. PUTNAM,

*Permanent Secretary.*



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## EXECUTIVE PROCEEDINGS.

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### 4. *Committee to memorialize Congress to take steps for the Preservation of Archæologic Monuments on the public lands.*

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WILLIAM SAUNDERS, Ottawa.

[1] All Committees are expected to present their reports to the COUNCIL not later than the fourth day of the meeting. Committees sending their reports to the Permanent Secretary one month before a meeting can have them printed for use at the meeting.

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C. H. MERRIAM.

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FOR THE

# BROOKLYN MEETING.

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F. **Zoology**—WM. LIBBEY, JR., Princeton, N. J.
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### TREASURER.

WILLIAM LILLY, Mauch Chunk, Pa. (Deceased.)

## MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

| MEETING. | DATE. | PLACE. | CHAIRMAN. | SECRETARY. | ASSIS'T SEC'Y. | TREASURER. |
|---|---|---|---|---|---|---|
| 1st | April 2, 1840, | Philadelphia, | Edward Hitchcock,* | L. C. Beck,* | | |
| 2d | April 5, 1841, | Philadelphia, | Benjamin Silliman,* | L. C. Beck,* | B. Silliman, jr.,* C. B. Trego,* | |
| 3d | April 25, 1842, | Boston, | S. G. Morton,* | C. T. Jackson,* | J. D. Whitney, M. B. Williams,* | |
| 4th | April 26, 1843, | Albany, | Henry D. Rogers,* | B. Silliman, jr.,* | | John Locke.* |
| 5th | May 8, 1844, | Washington, | John Locke,* | B. Silliman, jr.,* O. P. Hubbard, | | Douglas Houghton.* |
| 6th | April 30, 1845, | New Haven, | Wm. B. Rogers,* | B. Silliman, jr.,* J. Lawrence Smith,* | | Douglas Houghton.* |
| 7th | Sept. 2, 1846, | New York, | C. T. Jackson,* | B. Silliman, jr.,* | | E. C. Herrick.* |
| 8th | Sept. 20, 1847, | Boston, | Wm. B. Rogers,†* | Jeffries Wyman,* | | B. Silliman, jr.* |

* Deceased.

† Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

## OFFICERS OF THE MEETINGS OF THE ASSOCIATION.

[The number before the name is that of the meeting; the year of the meeting follows the name; the asterisk after a name indicates that the member is deceased.]

### PRESIDENTS.

1. W. C. Redfield,* 1848.
2. Joseph Henry,* 1849.

3, 4, 5. A. D. Bache,* March meeting, 1850, in absence of Joseph Henry.* August meeting, 1850. May meeting, 1851.

6. Louis Agassiz,* August meeting, 1851.

(No meeting in 1852).

7. Benjamin Pierce,* 1853.
8. James D. Dana, 1854.
9. John Torrey,* 1855.
10. James Hall, 1856.

11, 12. Alexis Caswell,* 1857, in place of J. W. Bailey,* deceased. 1858, in absence of Jeffries Wyman.

13. Stephen Alexander,* 1859.
14. Isaac Lea,* 1860.

(No meetings for 1861–65).

15. F. A. P. Barnard,* 1866.
16. J. S. Newberry,* 1867.
17. B. A. Gould, 1868.
18. J. W. Foster,* 1869.
19. T. Sterry Hunt,* 1870, in the absence of Wm. Chauvenet.*
20. Asa Gray,* 1871.
21. J. Lawrence Smith,* 1872.
22. Joseph Lovering,* 1873.
23. J. L. LeConte,* 1874.
24. J. E. Hilgard,* 1875.
25. William B. Rogers,* 1876.
26. Simon Newcomb, 1877.
27. O. C. Marsh, 1878.
28. G. F. Barker, 1879.
29. Lewis H. Morgan,* 1880.
30. G. J. Brush, 1881.
31. J. W. Dawson, 1882.
32. C. A. Young, 1883.
33. J. P. Lesley, 1884.
34. H. A. Newton, 1885.
35. Edward S. Morse, 1886.
36. S. P. Langley, 1887.
37. J. W. Powell, 1888.
38. T. C. Mendenhall, 1889.
39. G. Lincoln Goodale, 1890.
40. Albert B. Prescott, 1891.
41. Joseph LeConte, 1892.
42. William Harkness, 1893.
43. Daniel G. Brinton, 1894.

## VICE-PRESIDENTS.

There were no vice-presidents until the 11th meeting when there was a single vice-president for each meeting. At the 24th meeting the Association met in Sections A and B, each presided over by a vice-president. At the 31st meeting nine sections were organized, each with a vice-president as its presiding officer. In 1886, Section G (Microscopy) was given up. In 1892, the Section of Botany was organized as Section G.

### 1857–1874.

11. ALEXIS CASWELL,* 1857, acted as President.
12. JOHN E. HOLBROOK,* 1858, not present.
13. EDWARD HITCHCOCK,* 1859.
14. B. A. GOULD, 1860.
15. A. A. GOULD,*1866, in absence of R. W. GIBBES.
16. WOLCOTT GIBBS, 1867.
17. CHARLES WHITTLESEY,* 1868.
18. OGDEN N. ROOD, 1869.
19. T. STERRY HUNT,* 1870, acted as President.
20. G. F. BARKER, 1871.
21. ALEXANDER WINCHELL,* 1872.
22. A. H. WORTHEN,* 1873, not present.
23. C. S. LYMAN,* 1874.

### 1875–1881.

*Section A.—Mathematics, Physics and Chemistry.*

24. H. A. NEWTON, 1875.
25. C. A. YOUNG, 1876.
26. R. H. THURSTON, 1877, in the absence of E. C. PICKERING.
27. R. H. THURSTON, 1878.
28. S. P. LANGLEY, 1879.
29. ASAPH HALL, 1880.
30. WILLIAM HARKNESS, 1881, in the absence of A. M. MAYER.

*Section B.—Natural History.*

24. J. W. DAWSON, 1875.
25. EDWARD S. MORSE, 1876.
26. O. C. MARSH, 1877.
27. AUG. R. GROTE, 1878.
28. J. W. POWELL, 1879.
29. ALEXANDER AGASSIZ, 1880.
30. EDWARD T. COX, 1881, in the absence of GEORGE ENGELMANN.

### CHAIRMEN OF SUBSECTIONS, 1875–1881.

*Subsection of Chemistry.*

24. S. W. JOHNSON, 1875.
25. G. F. BARKER, 1876.
26. N. T. LUPTON, 1877.
27. F. W. CLARKE, 1878.
28. F. W. CLARKE, 1879, in the absence of IRA REMSEN.
29. J. M. ORDWAY, 1880.
30. G. C. CALDWELL, 1881, in the absence of W. R. NICHOLS.

*Subsection of Microscopy.*

25. R. H. WARD, 1876.
26. R. H. WARD, 1877.
27. R. H. WARD, 1878, in the absence of G. S. BLACKIE.
28. E. W. MORLEY, 1879.
29. S. A. LATTIMORE, 1880.
30. A. B. HERVEY, 1881.

*Subsection of Anthropology.*

24. LEWIS H. MORGAN,* 1875.
25. LEWIS H. MORGAN,* 1876.
26. DANIEL WILSON,* 1877, not present.
27. United with Section B.
28. DANIEL WILSON,* 1879.
29. J. W. POWELL, 1880.
30. GARRICK MALLERY, 1881.

*Subsection of Entomology.*

30. J. G. MORRIS, 1881.

## VICE-PRESIDENTS OF SECTIONS, 1882–

### *Section A.—Mathematics and Astronomy.*

31. W. A. ROGERS, 1882, in the absence of WILLIAM HARKNESS.
32. W. A. ROGERS, 1883.
33. H. T. EDDY, 1884.
34. WILLIAM HARKNESS, 1885, in the absence of J. M. VAN VLECK.
35. J. W. GIBBS, 1886.
36. J. R. EASTMAN, 1887, in place of W. FERREL, resigned.
37. ORMOND STONE, 1888.
38. R. S. WOODWARD, 1889.
39. S. C. CHANDLER, 1890.
40. E. W. HYDE, 1891.
41. J. R. EASTMAN, 1892.
42. C. L. DOOLITTLE, 1893.
43. G. C. COMSTOCK, 1894.

### *Section B.—Physics.*

31. T. C. MENDENHALL, 1882.
32. H. A. ROWLAND, 1883
33. J. TROWBRIDGE, 1884.
34. S. P. LANGLEY, 1885, in place of C. F. BRACKETT, resigned.
35. C. F. BRACKETT, 1886.
36. W. A. ANTHONY, 1887.
37. A. A. MICHELSON, 1888.
38. H. S. CARHART, 1889.
39. CLEVELAND ABBE, 1890.
40. F. E. NIPHER, 1891.
41. B. F. THOMAS, 1892.
42. E. L. NICHOLS, 1893.
43. WM. A. ROGERS, 1894.

### *Section C.—Chemistry.*

31. H. C. BOLTON, 1882.
32. E. W. MORLEY, 1883.
33. J. W. LANGLEY, 1884.
34. N. T. LUPTON, 1885, in absence of W. R. NICHOLS.*
35. H. W. WILEY, 1886.
36. A. B. PRESCOTT, 1887.
37. C. E. MUNROE, 1888.
38. W. L. DUDLEY, 1889.
39. R. B. WARDER, 1890.
40. R. C. KEDZIE, 1891.
41. ALFRED SPRINGER, 1892.
42. EDWARD HART, 1893.
43. T. H. NORTON, 1894.

### *Section D.—Mechanical Science and Engineering.*

31. W. P. TROWBRIDGE,* 1882.
32. DE VOLSON WOOD, 1883, absent, but place was not filled.
33. R. H. THURSTON, 1884.
34. J. BURKITT WEBB, 1885.
35. O. CHANUTE, 1886.
36. E. B. COXE, 1887.
37. C. J. H. WOODBURY, 1888.
38. JAMES E. DENTON, 1889.
39. JAMES E. DENTON. 1890.
40. THOMAS GRAY, 1891.
41. J. B. JOHNSON, 1892.
42. S. W. ROBINSON, 1893.
43. MANSFIELD MERRIMAN, 1894.

## VICE-PRESIDENTS OF SECTIONS, CONTINUED.

*Section E.—Geology and Geography.*

31. E. T. COX, 1882.
32. C. H. HITCHCOCK, 1883.
33. N. H. WINCHELL, 1884.
34. EDWARD ORTON, 1885.
35. T. C. CHAMBERLIN, 1886.
36. G. K. GILBERT, 1887.
37. GEORGE H. COOK,* 1888.
38. CHARLES A. WHITE, 1889.
39. JOHN C. BRANNER, 1890.
40. J. J. STEVENSON, 1891.
41. H. S. WILLIAMS, 1892.
42. CHARLES D. WALCOTT, 1893.
43. SAMUEL CALVIN, 1894.

*Section F.—Biology.*

31. W. H. DALL, 1882.
32. W. J. BEAL, 1883.
33. E. D. COPE, 1884.
34. T. J. BURRILL, 1885, in the absence of B. G. WILDER.
35. H. P. BOWDITCH, 1886.
36. W. G. FARLOW, 1887.
37. C. V. RILEY, 1888.
38. GEORGE L. GOODALE, 1889.
39. C. S. MINOT, 1890.
40. J. M. COULTER, 1891.
41. S. H. GAGE, 1892.

*Section F.— Zoölogy.*

42. HENRY F. OSBORN, 1893.
43. ——— ——— 1894, in place of S. H. SCUDDER, resigned.

*Section G.—Microscopy.*

31. A. H. TUTTLE, 1882.
32. J. D. COX, 1883.
33. T. G. WORMLEY, 1884.
34. S. H. GAGE, 1885.

(Section united with F in 1886.)

*Section G.—Botany.*

42. CHARLES E. BESSEY, 1893.
43. L. M. UNDERWOOD, 1894.

*Section H.—Anthropology.*

31. ALEXANDER WINCHELL,* 1882.
32. OTIS T. MASON, 1883.
33. EDWARD S. MORSE, 1884.
34. J. OWEN DORSEY, 1885, in absence of W. H. DALL.
35. HORATIO HALE, 1886.
36. D. G. BRINTON, 1887.
37. CHARLES C. ABBOTT, 1888.
38. GARRICK MALLERY, 1889.
39. FRANK BAKER, 1890.
40. JOSEPH JASTROW, 1891.
41. W. H. HOLMES, 1892.
42. J. OWEN DORSEY, 1893.
43. FRANZ BOAS, 1894.

*Section I.—Economic Science and Statistics.*

31. E. B. ELLIOTT,* 1882.
32. FRANKLIN B. HOUGH,* 1883.
33. JOHN EATON, 1884.
34. EDWARD ATKINSON, 1885.
35. JOSEPH CUMMINGS,* 1886.
36. H. E. ALVORD, 1887.
37. CHARLES W. SMILEY, 1888.
38. CHARLES S. HILL, 1889.
39. J. RICHARDS DODGE, 1890.
40. EDMUND J. JAMES, 1891.
41. LESTER F. WARD, 1892, in place of S. DANA HORTON, resigned.
42. WILLIAM H. BREWER, 1893.
43. HENRY FARQUHAR, 1894.

## SECRETARIES.

*General Secretaries*, 1848–

1. Walter R. Johnson,* 1848.
2. Eben N. Horsford,* 1849, in the absence of Jeffries Wyman.
3. L. R. Gibbs, 1850, in absence of E. C. Herrick.
4. E. C. Herrick,* 1850.
5. William B. Rogers,* 1851, in absence of E. C. Herrick.
6. William B. Rogers,* 1851.
7. S. St. John,* 1853, in absence of J. D. Dana.
8. J. Lawrence Smith,* 1854.
9. Wolcott Gibbs, 1855.
10. B. A. Gould, 1856.
11. John LeConte, 1857.
12. W. M. Gillespie,* 1858, in absence of Wm. Chauvenet.
13. William Chauvenet,* 1859.
14. Joseph LeConte, 1860.
15. Elias Loomis,* 1866, in the absence of W. P. Trowbridge.
16. C. S Lyman,* 1867.
17. Simon Newcomb, 1868, in place of A. P. Rockwell, called home.
18. O. C. Marsh, 1869.
19. F. W. Putnam, 1870, in absence of C. F. Hartt.
20. F. W. Putnam, 1871.
21. Edward S. Morse, 1872.
22. C. A. White, 1873.
23. A. C. Hamlin, 1874.
24. S. H. Scudder, 1875.
25. T. C. Mendenhall, 1876.
26. Aug. R. Grote, 1877.
27. H. C. Bolton, 1878.
28. H. C. Bolton, 1879, in the absence of George Little.
29. J. K. Rees, 1880.
30. C. V. Riley, 1881.
31. William Saunders, 1882.
32. J. R. Eastman, 1883.
33. Alfred Springer, 1884.
34. C. S. Minot, 1885.
35. S. G Williams, 1886.
36. William H. Pettee, 1887.
37. Julius Pohlman, 1888.
38. C. Leo Mees, 1889.
39. H. C. Bolton, 1890.
40. H. W. Wiley, 1891.
41. A. W. Butler, 1892.
42. T. H. Norton, 1893.
43. H. L. Fairchild, 1894.

*Permanent Secretaries*, 1851–

5–7. Spencer F. Baird,* 1851–3.
8–17. Joseph Lovering,* 1854–68.
18. F. W. Putnam, 1869, in the absence of J. Lovering.
19–21. Joseph Lovering,* 1870–72.
22–23. F. W. Putnam, 1873–74.
24–28. F. W. Putnam, 1875–79.
29–33. F. W. Putnam, 1880–84.
34–38. F. W. Putmam, 1885–89.
39–43. F. W. Putnam, 1890–94.

*Assistant General Secretaries*, 1882-1887.

31. J. R. Eastman, 1882.
32. Alfred Springer, 1883.
33. C. S. Minot, 1884, in the absence of E. S. Holden.
34. S. G. Williams, 1885, in the absence of C. C. Abbott.
35. W. H. Pettee, 1886.
36. J. C. Arthur, 1887.

*Secretaries of the Council*, 1888–

37. C. Leo Mees, 1888.
38. H. C. Bolton, 1889.
39. H. W. Wiley, 1890.
40. A. W. Butler, 1891.
41. T. H. Norton, 1892.
42. H. Leroy Fairchild, 1893.
43. Jas. Lewis Howe, 1894.

*Secretaries of Section A.—Mathematics, Physics and Chemistry*, 1875–81.

24. S. P. LANGLEY, 1875. / T. C. MENDENHALL, 1875.
25. A. W. WRIGHT, 1876.
26. H. C. BOLTON, 1877.
27. F. E. NIPHER, 1878.
28. J. K. REES, 1879.
29. H. B. MASON, 1880.
30. E. T. TAPPAN, 1881, in the absence of JOHN TROWBRIDGE.

*Secretaries of Section B.— Natural History*, 1875-81.

24. EDWARD S. MORSE, 1875.
25. ALBERT H. TUTTLE, 1876.
26. WILLIAM H. DALL, 1877.
27. GEORGE LITTLE, 1878.
28. WILLIAM H. DALL, 1879, in the absence of A. C. WETHERBY.
29. CHARLES V. RILEY, 1880.
30. WILLIAM SAUNDERS, 1881.

## SECRETARIES OF SUBSECTIONS, 1875-81.

*Subsection of Chemistry.*

24. F. W. CLARKE, 1875.
25. H. C. BOLTON, 1876.
26. P. SCHWEITZER, 1877.
27. A. P. S. STUART, 1878.
28. W. R. NICHOLS,* 1879.
29. C. E. MUNROE, 1880.
30. ALFRED SPRINGER, 1881, in the absence of R. B. WARDER.

*Subsection of Entomology.*

30. B. P. MANN, 1881.

*Subsection of Anthropology.*

24. F. W. PUTNAM, 1875.
25. OTIS T. MASON, 1876.
26, 27. United with Section B.
28, 29, 30. J. G. HENDERSON, 1879-81.

*Subsection of Microscopy.*

25. E. W. MORLEY, 1876.
26. T. O. SOMMERS, JR., 1877.
27. G. J. ENGELMANN, 1878.
28, 29. A. B. HERVEY, 1879-1880.
30. W. H. SEAMAN, 1881, in the absence of S. P. SHARPLES.

## SECRETARIES OF THE SECTIONS, 1882–

*Section A.—Mathematics and Astronomy.*

31. H. T. EDDY, 1882.
32. G. W. HOUGH, 1883, in the absence of W. W. JOHNSON.
33. G. W. HOUGH, 1884.
34. E. W. HYDE, 1885.
35. S. C. CHANDLER, 1886.
36. H. M. PAUL, 1887.
37. C. C. DOOLITTLE, 1888.
38. G. C. COMSTOCK, 1889.
39. W. W. BEMAN, 1890.
40. F. H. BIGELOW, 1891.
41. WINSLOW UPTON, 1892.
42. C. A. WALDO, 1893, in place of A. W. PHILLIPS, not present.
43. W. W. BEMAN, 1894.

*Section B.—Physics.*

31. C. S. HASTINGS, 1882.
32. F. E. NIPHER, 1883, in the absence of C. K. WEAD.
33. N. D. C. HODGES, 1884.
34. B. F. THOMAS, 1885, in place of A. A. MICHELSON, resigned.
35. H. S. CARHART, 1886.
36. C. LEO MEES, 1887.
37. ALEX. MACFARLANE, 1888.
38. E. L. NICHOLS, 1889.
39. E. M. AVERY, 1890.
40. ALEX. MACFARLANE, 1891.
41. BROWN AYRES, 1892.
42. W. LECONTE STEVENS, 1893.
43. B. W. SNOW, 1894.

## SECRETARIES OF THE SECTIONS, CONTINUED.

*Section C.—Chemistry.*

31. ALFRED SPRINGER, 1882.
32. J. W. LANGLEY, 1883.
    W. MCMURTRIE, "
33. H. CARMICHAEL, 1884, in the absence of R. B. WARDER.
34. F. P. DUNNINGTON, 1885.
35. W. MCMURTRIE, 1886.
36. C. S. MABERY, 1887.
37. W. L. DUDLEY, 1888.
38. EDWARD HART, 1889.
39. W. A. NOYES, 1890.
40. T. H. NORTON, 1891.
41. JAMES LEWIS HOWE, 1892.
42. H. N. STOKES, 1893, in place of J. U. NEF, not present.
43. S. M. BABCOCK, 1894.

*Section D.—Mechanical Science and Engineering.*

31. J. BURKITT WEBB, 1882, in the absence of C. R. DUDLEY.
32. J. BURKITT WEBB, 1883, pro tempore.
33. J. BURKITT WEBB, 1884.
34. C. J. H. WOODBURY, 1885.
35. WILLIAM KENT, 1886.
36. G. M. BOND, 1887.
37. ARTHUR BEARDSLEY, 1888.
38. W. B. WARNER, 1889.
39. THOMAS GRAY, 1890.
40. WILLIAM KENT, 1891.
41. O. H. LANDRETH, 1892.
42. D. S. JACOBUS, 1893.
43. JOHN H. KINEALY, 1894.

*Section E.—Geology and Geography.*

31. H. S. WILLIAMS, 1882, in the absence of C. E. DUTTON.
32. A. A. JULIEN, 1883.
33. E. A. SMITH, 1884.
34. G. K. GILBERT, 1885, in the absence of H. C. LEWIS.
35. E. W. CLAYPOLE, 1886.
36. W. M. DAVIS, 1887, in the absence of T. B. COMSTOCK.
37. JOHN C. BRANNER, 1888.
38. JOHN C. BRANNER, 1889.
39. SAMUEL CALVIN, 1890.
40. W J MCGEE, 1891.
41. R. D. SALISBURY, 1892.
42. W. H. HOBBS, 1893, in place of R. T. HILL, resigned.
43. ——— ——— 1894, in place of W. M. DAVIS, resigned.

*Section F.—Biology*, 1882–92.

31. WILLIAM OSLER, 1882, in the absence of C. S. MINOT.
32. S. A. FORBES, 1883.
33. C. E. BESSEY, 1884.
34. J. A. LINTNER, 1885, in place of C. H. FERNALD, resigned.
35. J. C. ARTHUR, 1886.
36. J. H. COMSTOCK, 1887.
37. B. H. FERNOW, 1888.
38. A. W. BUTLER, 1889.
39. J. M. COULTER, 1890.
40. A. J. COOK, 1891.
41. B. D. HALSTED, 1892.

*Section F.—Zoölogy.*

42. L. O. HOWARD, 1893.
43. WM. LIBBY, jr. 1894.

### SECRETARIES OF THE SECTIONS, CONTINUED.

*Section G.—Microscopy*, 1882–85.

31. ROBERT BROWN, JR., 1882.
32. CARL SEILER, 1883.
33. ROMYN HITCHCOCK, 1884.
34. W. H. WALMSLEY, 1885.

*Section G.—Botany.*

42. B. T. GALLOWAY, 1893, in place of F. V. COVILLE, not present.
43. CHARLES R. BARNES, 1894.

*Section H.—Anthropology.*

31. OTIS T. MASON, 1882.
32. G. H. PERKINS, 1883.
33. G. H. PERKINS, 1884, in the absence of W. H. HOLMES.
34. ERMINNIE A. SMITH,* 1885.
35. A. W. BUTLER, 1886.
36. CHARLES C. ABBOTT, 1887, in absence of F. W. LANGDON.
37. FRANK BAKER, 1888.
38. W. M. BEAUCHAMP, 1889.
39. JOSEPH JASTROW, 1890.
40. W. H. HOLMES, 1891.
41. W. M. BEAUCHAMP, 1892, in place of S. CULIN, resigned.
42. WARREN K. MOOREHEAD, 1893.
43. A. F. CHAMBERLIN, 1894.

*Section I.—Economic Science and Statistics.*

31. { FRANKLIN B. HOUGH,* 1882. / J. RICHARDS DODGE, 1882. }
32. JOSEPH CUMMINGS,* 1883.
33. CHARLES W. SMILEY, 1884.
34. CHARLES W. SMILEY, 1885, in place of J. W. CHICKERING.
35. H. E. ALVORD, 1886.
36. W. R. LAZENBY, 1887.
37. CHARLES S. HILL, 1888.
38. J. RICHARDS DODGE, 1889.
39. B. E. FERNOW, 1890.
40. B. E. FERNOW, 1891.
41. HENRY FARQUHAR, 1892, in place of L. F. WARD made Vice-president.
42. NELLIE S. KEDZIE, 1893.
43. MANLEY MILES, 1894.

### TREASURERS.

1\. JEFFRIES WYMAN*, 1848.
2\. A. L. ELWYN,* 1849.
3\. ST. J. RAVENEL,* 1850, in the absence of A. L. ELWYN.
4\. A. L. ELWYN,* 1850.
5\. SPENCER F. BAIRD,* 1851, in absence of A. L. ELWYN.
6–7. A. L. ELWYN,* 1851–1853.
8\. J. L. LECONTE,* 1854, in absence of A. L. ELWYN.
9–19. A. L. ELWYN,* 1855–1870.
20–30. WILLIAM S. VAUX,* 1871–1881.
32–42. WILLIAM LILLY,* 1882–1893.

| MEETINGS. | PLACE. | DATE. | MEMBERS IN ATTENDANCE. | NUMBER OF MEMBERS. |
|---|---|---|---|---|
| 1. | Philadelphia | Sept. 20, 1848 | ? | 461 |
| 2. | Cambridge | Aug. 14, 1849 | ? | 540 |
| 3. | Charleston | Mar. 12, 1850 | ? | 622 |
| 4. | New Haven | Aug. 19, 1850 | ? | 704 |
| 5. | Cincinnati | May 5, 1851 | 87 | 800 |
| 6. | Albany | Aug. 19, 1851 | 194 | 769 |
| 7. | Cleveland | July 28, 1853 | ? | 940 |
| 8. | Washington | April 26, 1854 | 168 | 1004 |
| 9. | Providence | Aug. 15, 1855 | 166 | 605 |
| 10. | 2nd Albany | Aug. 20, 1856 | 381 | 722 |
| 11. | Montreal | Aug. 12, 1857 | 351 | 946 |
| 12. | Baltimore | April 28, 1858 | 190 | 962 |
| 13. | Springfield | Aug. 3, 1859 | 190 | 862 |
| 14. | Newport | Aug. 1, 1860 | 135 | 644 |
| 15. | Buffalo | Aug. 15, 1866 | 79 | 637 |
| 16. | Burlington | Aug. 21. 1867 | 73 | 415 |
| 17. | Chicago | Aug. 5, 1868 | 259 | 686 |
| 18. | Salem | Aug. 18, 1869 | 244 | 511 |
| 19. | Troy | Aug. 17, 1870 | 188 | 536 |
| 20. | Indianapolis | Aug. 16, 1871 | 196 | 668 |
| 21. | Dubuque | Aug. 15, 1871 | 164 | 610 |
| 22. | Portland | Aug. 20, 1873 | 195 | 670 |
| 23. | Hartford | Aug. 12, 1874 | 224 | 722 |
| 24. | Detroit | Aug. 11, 1875 | 165 | 807 |
| 25. | 2nd Buffalo | Aug. 23, 1876 | 215 | 867 |
| 26. | Nashville | Aug. 29, 1877 | 173 | 953 |
| 27. | St. Louis | Aug. 21, 1878 | 184 | 962 |
| 28. | Saratoga | Aug. 27, 1879 | 256 | 1030 |
| 29. | Boston | Aug. 25, 1880 | 997 | 1555 |
| 30. | 2nd Cincinnati | Aug. 17, 1881 | 500 | 1699 |
| 31. | 2nd Montreal | Aug. 23, 1882 | 937 | 1922 |
| 32. | Minneapolis | Aug. 15, 1883 | 328 | 2033 |
| 33. | 2nd Philadelphia | Sept. 3, 1884 | 1261* | 1981 |
| 34. | Ann Arbor | Aug. 26, 1885 | 364 | 1956 |
| 35. | 3d Buffalo | Aug. 18, 1886 | 445 | 1886 |
| 36. | New York | Aug. 10, 1887 | 729 | 1956 |
| 37. | 2nd Cleveland | Aug. 14, 1888 | 342 | 1964 |
| 38. | Toronto | Aug. 26, 1889 | 424 | 1952 |
| 39. | 2d Indianapolis | Aug. 19, 1890 | 364 | 1944 |
| 40. | 2d Washington | Aug. 19, 1891 | 653† | 2054 |
| 41. | Rochester | Aug. 17, 1892 | 456 | 2037 |
| 42. | Madison | Aug. 17, 1893 | 290 | 1939 |

*Including members of the British Association and other foreign guests.
†Including twenty-four foreign Honorary members for the meeting.

# COMMONWEALTH OF MASSACHUSETTS.

## IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

# AN ACT

### To Incorporate the "American Association for the Advancement of Science."

*Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:*

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be, possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John F. Sanford, *Speaker.*

In Senate, March 17, 1874.
Passed to be enacted.
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

Secretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

# CONSTITUTION

OF THE

# AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

**Incorporated by Act of the General Court of the Commonwealth of Massachusetts.**

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OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, Corresponding Members and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, and election by the Council.

ART. 4. Fellows shall be elected by the Council from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members of the Council at a designated meeting of the Council.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Council and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of Fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election. Corresponding Members shall consist of such scientists not re-

siding in America as may be elected by the Council, and their number shall be limited to fifty. Corresponding Members shall be entitled to all the privileges of members and to the annual volumes of Proceedings published subsequent to their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected. The Council shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Council made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice-President from each section, a Permanent Secretary, a General Secretary, a Secretary of the Council, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Council. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Council shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of

these sessions. He shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting.

ART. 13. The Secretary of the Council shall keep the records of the Council. He shall give to the Secretary of each Section the titles of papers assigned to it by the Council. He shall receive proposals for membership and bring them before the Council.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Council. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Council, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Council, and shall pay over to the Treasurer such unexpended funds as the Council may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Council. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Council, and may employ one or more clerks at such compensation as may be agreed upon by the Council.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Council. He shall annually present to the Council an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Council, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Council.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Council as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Secretary of the Council, and Treasurer, shall be filled by nomination of the Council and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Council shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Secretary of the Council, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Council, provided there are at least five, shall form a quorum for the transaction of business. The Council shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Council shall be held in the council room at 9 o'clock, A. M., on each day of the meeting of the Association. Special meetings of the Council may be called at any time by the President. The Council shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Council. The Council shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other proceedings shall be published, and have the general direction of the pub-

lications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; elect members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association. The Council shall receive all reports of Special Committees and decide upon them, and only such shall be read in General Session as the Council shall direct. The Council shall appoint at each meeting the following sub-committees who shall act, subject to appeal to the whole Council, until their successors are appointed at the following meeting: 1, on Papers and Reports; 2, on Members; 3, on Fellows.

ART. 19. The Nominating Committee shall consist of the Council, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

### MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Council may designate.

ART. 21. A General Session shall be held at 10 o'clock A. M., on the first day of the meeting, and at such other times as the Council may direct.

### SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into Sections, namely:— A, *Mathematics and Astronomy*; B, *Physics*; C, *Chemistry, including its application to agriculture and the arts*; D, *Mechanical Science and Engineering*; E, *Geology and Geography*; F, *Zoölogy*; G, *Botany*; H, *Anthropology*; I, *Economic Science and Statistics*. The Council shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

**Art. 23.** Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary and the Vice President and Secretary of the preceding meeting shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

**Art. 24.** The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

**Art. 25.** A paper shall not be read in any Section or Subsection until it has been received from the Council and placed on the programme of the day by the Sectional Committee.

### Sectional Committees.

**Art. 26.** The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Council. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Council with the reasons why it was refused.

**Art. 27.** The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

### Papers and Communications.

**Art. 28.** All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Council to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

PRINTED PROCEEDINGS.

ART. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press, and these must be in the hands of the Secretaries of the Sections before the final adjournment of the meeting, otherwise only the titles will appear in the printed volume. The Council shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Council. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Council. The Council shall also designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered

to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Council.

### Admission Fee and Assessments.

Art. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 35. The annual assessment for members and fellows shall be three dollars.

Art. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which, during the life of the member, shall form a part of the general fund of the Association; but, after his death, shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Council.

Art. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

### Accounts.

Art. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Council.

### Alterations of the Constitution.

Art. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

# MEMBERS

OF THE

# AMERICAN ASSOCIATION

FOR THE

## ADVANCEMENT OF SCIENCE.[1]

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### PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).

LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28). (Died Dec. 1, 1893.)

HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).

### CORRESPONDING MEMBERS.[3]

Monselise, Prof. G., Via Principe Amedeo I, Milano, Italy (40). **C**

Warington, Robert, F.R.S., Rothamsted, Harpenden, England (40). **C**

### MEMBERS.[4]

Acker, Dr. Geo. N., 913 16th St., Washington, D. C. (40). **F**

Adriance, John S., 231 Broadway, New York, N. Y. (39). **C**

Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).

Aitkin, Miss Clara I., 210 Madison St., Brooklyn, N. Y. (40). **H**

Aitkin, Miss Helen J., 210 Madison St., Brooklyn, N. Y. (40). **E H**

[1] The numbers in parentheses indicate the meeting at which the member was elected. The black letters at the end of line indicate the sections to which members elect to belong. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes to which he may be entitled, bound in cloth, or by the payment of twenty dollars, to such volumes bound in half morocco.*

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

The names of Patrons are to remain permanently on the list.

[3] See ARTICLE VI of the Constitution.

[4] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The income of the money derived from a Life Membership is used for the general purposes of the Association during the life of the member; afterwards it is to be used to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Alden, Jno., Pacific Mills, Lawrence, Mass. (36).
Aldis, Owen F., 230 Monadnock Block, Chicago, Ill. (41). H
Allderdice, Wm. H., Ass't Eng. U. S. N., U. S. Naval Academy, Annapolis, Md. (33). D
Allen, J. M., Hartford, Conn. (22). D
Allen, W. F., 24 Park Place, New York, N. Y. (36).
Allen, Walter S., New Bedford, Mass. (39). C I
Alpaugh, Edwin K., Huntington, Ind. (41). E
Anderson, Alexander D., Washington, D. C. (38).
Andrews, E. R., Rochester, N. Y. (41).
Angell, Geo. W. J., 44 Hudson St., New York, N. Y. (36).
Appleton, Rev. Edw. W., D.D., Ashbourne, Montgomery Co., Pa. (28).
Archambault, U. E., P. O. Box 1944, Montreal, P. Q., Can. (31).
Archbold, Dr. George, 121 and 123 Front St., New York, N. Y. (40).
Armstrong, Mrs. Lucius H., St. Nicholas, Duval Co., Fla. (30).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26). D
Atwood, E. S., East Orange, N. J. (29). F
Auhagen, Wilhelm, Mt. Pleasant, 17th St. ext., Washington, D. C. (40).
Austin, H. W., No. 3 B St., S. E., Washington, D. C. (40). C
Avery, Robert Stanton, 320 A St., S. E., Washington, D. C. (40). A
AVERY, SAMUEL P., 4 E. 38th St., New York, N. Y. (36).
Ayer, Edward Everett, Room 12, The Rookery, Chicago, Ill. (37). H
Ayres, Horace B., Allamuchy, N. J. (40).

Babcock, Wm. H., Washington, D. C. (40).
Bacon, Chas. A., Beloit, Wis. (36). A
Baker, Prof. Arthur Latham, 28 Strathallan Park, Rochester, N. Y. (41). A B
Baker, Charles S., Rochester, N. Y. (41). C D
Baker, Richard D., 112 So. 21st St., Philadelphia, Pa. (33). E C
Baker, Wm. G., 234 E. 18th St., New York, N. Y. (36).
Balderston, C. Canby, Westtown, Chester Co., Pa. (33). B
Baldwin, Miss Mary A., 28 Fulton St., Newark, N. J. (31). E
Baldwin, Mrs. G. H., 3 Madison Ave., Detroit, Mich. (34). H
Bancroft, Alonzo C., Elma, Erie Co., N. Y. (41).
Banes, Charles H., 1107 Market St., Philadelphia, Pa. (31). D
BANGS, LEMUEL BOLTON, M.D., 127 E. 34th St., New York, N. Y. (36).
Barber, D. H., Marion, Iowa (37).
Barclay, Robert, A.M., M.D., 3211 Lucas Ave., St. Louis, Mo. (30).
BARGE, B. F., Mauch Chunk, Pa. (33).
Barker, Francis C., 200 D St., N. W., Washington, D. C. (40). E
Barker, Mrs. Martha M., 26 Eleventh St., Lowell, Mass. (31). E H
Barnes, Hon. Willis L., Charlestown, Ind. (40). A B
Barnhart, Arthur M., 185 Monroe St., Chicago, Ill. (42).
Barnum, Miss Charlotte C., 144 Humphrey St., New Haven, Conn. (36). A
Barrett, Fred. P., Gainesville, Wyoming Co., N. Y. (40). E
Barringer, Daniel M., Bullitt Building, Philadelphia, Pa. (40). C E

Barrows, Walter B., Dept. of Agric., Washington, D. C. (40). F
Bartley, Elias H., M.D., 21 Lafayette Ave., Brooklyn, N. Y. (33). C
Bascom, Miss Florence, Ohio State Univ., Columbus, Ohio. (42). E
Baskerville, Charles, Univ. of North Carolina, Chapel Hill, N. C. (41). C
Bastin, Edson Sewell, 2421 Dearborn St., Chicago, Ill. (39).
Bates, Wm. W., Com. of Navigation, Washington, D. C. (38).
Batterson, J. G., Hartford, Conn. (23).
Bausch, Henry, P. O. Drawer 1033, Rochester, N. Y. (41).
Baxter, James N., care H. E. and C. Baxter, cor. Division and Bedford Sts., Brooklyn, N. Y. (36).
Bay, J. Christian, Bacteriologist of the Iowa State Board of Health, Ames, Iowa. (42). G
Beach, Spencer Ambrose, N. Y. Experiment Station, Geneva, N. Y. (41). G
Beall, Fielder M. M., Lieut. 18th Infantry, U. S. A., Fort Clark, Brackettville, Texas (40).
Bean, B. A., Ass't Curator, Dept. of Fishes, National Museum, Washington, D. C. (40). F
Bean, Thos. E., Box 441, Galena, Ill. (28). F
Beaver, Daniel B. D., M.D., 150 North 6th St., Reading, Pa. (39).
Bechdolt, Adolphus F., Supt. City Schools, Mankato. Minn. (32). E B F
Becher, Franklin A., 406 Irving Place, Milwaukee, Wis. (41). I A
Bedell, Frederick, Ph.D., Cornell Univ., Ithaca, N. Y. (41). B A
Bell, C. M., M.D., 320 Fifth Ave., New York, N. Y. (36).
Belt, R. V., 1314 10th St., N. W., Washington, D. C. (40).
Bemis, Edward W., Assoc. Prof. of Economics, Univ. of Chicago, Chicago, Ill. (41). I
Benner, Henry, Chicago Manual Training School, Michigan Ave. and 12th St., Chicago, Ill. (40). A
Bennett, Charles M., Logansport, Ind. (39).
Benton, George W., High School, Indianapolis, Ind. (39). C
Berry, Daniel, M.D., Carmi, White Co., Ill. (41). B C E
Beveridge, David, Newburgh, N. Y. (33). I
Biddle, James G., 1010 Chestnut St., Philadelphia, Pa. (39).
Bien, Julius, 140 Sixth Ave., New York, N. Y. (34). E H
Biggar, Hamilton F., M.D., 176 Euclid Ave., Cleveland, Ohio (40). B F
Birge, Prof. Edw. A., Univ. of Wis., Madison, Wis. (42). F
Biscoe, Prof. Thomas Dwight, 404 Front St., Marietta, Ohio (41). G
Bishop, Heber R., Mills Building, New York, N. Y. (36).
Blair, Mrs. Eliza N., Manchester, N. H. (40).
Blatchford, Eliphalet W., 375 No. La Salle St., Chicago, Ill. (17). F
Bleile, Albert M., M.D., 342 S. Fourth St., Columbus, Ohio (37). F
Blish, W. G., Niles, Mich. (33). B D
Blount, Henry F., "The Oaks," Washington, D. C. (32). I B
Blount, Mrs. Lucia E., "The Oaks," Washington, D. C. (34). H I
Bogue, Rev. Horace P. V., Avon, N. Y. (41). H I

Booraem, J. V. V., 204 Lincoln Place, Brooklyn, N. Y. (36).
Booth, Miss Mary A., Longmeadow, Mass. (34). F I G
Bourland, Addison M., M.D., Van Buren, Ark. (29). C E F
Bouton, Chas. L., M.S., 2909 Park Ave., St. Louis, Mo. (40). A D
Bowers, Miss Virginia K., 61 3d St., Newport, Ky. (27). F H B C
Bowman, Chas. G., Lt. U. S. N., Naval Acad., Annapolis, Md. (33).
Boyer, Jerome L., Superintendent Chestnut Hill Iron Ore Co., Reading, Pa. (35). D
BRACKENRIDGE, GEO. W., San Antonio, Texas (41). I
Bradley, Charles S., Avon, N. Y. (40).
Brannon, M. A., Fort Wayne, Ind. (39). F
Bray, Prof. C. D., College Hill, Mass. (29). D B
Brayton, Miss Sarah H., M.D., Evanston, Ill. (33).
Breckenridge, Prof. Lester P., Agricultural College, Mich. (41).
Breckinridge, J. C., Inspector General U. S. A, Inspector General's Office, War Dept., Washington, D. C. (40). E
Brewer, Prof. Charles E., Wake Forest, N. C. (40).
Brice, Judge Albert G., 19 Camp St., New Orleans, La. (32). H
Brigham, Prof. Albert P., Hamilton, Madison Co., N. Y. (41).
Bristol, Wm. H., Stevens Institute, Hoboken, N. J. (36). A B D
Britton, Wiley, Pension Office, Washington, D. C. (40). F
Bromwell, Wm., Museum of Hygiene, 1707 New York Ave, Washington, D. C. (40).
Bronson, Henry, 1198 Chapel Street, New Haven, Conn. (41). I
Brooks, Prof. Wm. P., Amherst, Mass. (38). C F
Broomall, Hon. John M., Media, Delaware Co., Pa. (23). A
Brown, Prof. Charles Sumner, Rose Polytechnic Institute, Terre Haute, Ind. (39). D
Brown, C. Newton, Ohio State Univ., Columbus, Ohio (34).
Brown, Henry A., Saxonville, Mass. (38). I
Brown, Jonathan, 390 Broadway, Somerville, Mass. (29).
Brown, Samuel B., Morgantown, W. Va. (40). E
Brown, Prof. W. G., Lexington, Va. (40).
Brownell, Prof. Walter A., 905 University Avenue, Syracuse, N. Y. (30). E B C
Brunk, Thos. L., College Park, Md. (40). F
Bryant, Miss D. L., Greensboro, N. C. (42). E
Buckingham, Chas. L., 195 Broadway, New York, N. Y. (28).
Buffum, Prof. Burt C., State Univ., Laramie, Wyo. (42). G
Buffum, Miss Fannie A., Linden, Mass. (29). E C
Burke, William, U. S. Patent Office, Washington, D. C. (28).
Burman, Rev. W. A., Winnepeg, Manitoba (38). F H
Burnett, Edgar A., Agricultural College, Mich. (41).
Burns, Prof. James A., Box 206, Atlanta, Ga. (32). C E I
Burr, Mrs. Laura E., Commercial Hotel, Lansing, Mich. (34). B
Burton, Prof. Alfred E., Mass. Inst. of Tech., Boston, Mass. (40). E
Burwell, Arthur W., Ph.D., 208 Superior St., Cleveland, Ohio (37).

Bush, Rev. Stephen, D.D., Waterford, N. Y. (19). E H
Byrnes, Eugene A., Principal Examiner of Metallurgy and Chemist of the Patent Office, Washington, D. C. (40).

Cabot, John W., Box 440, Pottstown, Pa. (35). D
Calder, Edwin E., 15 Board of Trade Building, Providence, R. I. (29). C
Caldwell, Wm. H., State College, Centre Co., Pa. (37). I F
Calkins, Dr. Marshall, Springfield, Mass. (29).
Campbell, H. D., Ph.D., Lexington, Va. (40). F
Campbell, Jos. Addison, 5103 Main St., Germantown, Pa. (33).
Campbell, Prof. John L., Crawfordsville, Ind. (39). B
Campbell, John T., Rockville, Ind. (39).
Campbell, Wm. A., M.D., Ann Arbor, Mich. (34). F B
Cannon, George L., jr., High School, Denver, Col. (39). F H
Cardeza, John M., M.D., Claymont, Del. (33). E
Carleton, M. A., Agric'l Exper. Station, Manhattan, Kan. (42). G
Carpenter, Geo. O., jr., care of St. Louis Lead and Oil Co., St. Louis, Mo. (29).
Carr, Oma, Ass't Chem. Dept. of Agric., Washington, D. C. (40).
CARTER, JAMES C., 277 Lexington Ave., New York, N. Y. (36).
Carter, James Madison G., M.D., Waukegan, Ill. (39). F
Carter, John E., Knox and Coulter Sts., Germantown, Pa. (33). B H
Carus, Paul, Ph.D., La Salle, Ill. (40).
Cary, Albert A., care Abendroth and Root M'f'g Co., cor. West and Noble Sts., Brooklyn, E. D., N. Y. (36). D
Catlin, Charles A., 133 Hope St., Providence, R. I. (33). C
Chadbourn, Erlon R., Lewiston, Me. (29).
Chamberlain, Prof. Joseph R., Raleigh, N. C. (41). F
Chandler, George V., Room 85, Patent Office, Washington, D. C. (40). B C
Chandler, John R., Ph.D. (36). F H
Chaplin, Prof. Winfield S., care Washington Univ., St. Louis, Mo. (37). D A
Chapman, Mrs. Etta L., 1207 L St., N. W., Washington, D. C. (40).
Chapman, W. Albert, M. E., Yellville, Ark. (40). C
Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26). A B D
Chase, Rev. E. B., Lake City, Minn. (37).
Chase, R. Stuart, 53 Summer St., Haverhill, Mass. (18). F
Chatfield, A. F., 23 Nahant St., Lynn, Mass. (29).
Cheney, Lellen Sterling, 519 Lake St., Madison, Wis. (42). G
Chester, Commander Colby M., U. S. N., U. S. Naval Academy, Annapolis, Md. (28). E
Child, Wm. Addison, M.A., Ontario Rolling Mill Co., Swansea, Ont., Can. (38). C H D
Christian, Ira W., Noblesville, Ind. (39).
Christie, James, Pencoyd, Pa. (33). D
Christy, Prof. Samuel B., Box 41, Berkeley, Cal. (35). D
Chrystie, Wm. F., Hastings-on-Hudson, New York, N. Y. (36).

Church, Royal Tyler, Turin, Lewis Co., N. Y. (38). D F
Clancy, Michael Albert, 1426 Corcoran St., Washington, D. C. (40). H
Clapp, Geo. H., 116 Water St., Pittsburg, Pa. (33). H C
Clark, Alex. S., Westfield, N. J. (33).
Clark, Edward, 417 Fourth St., Washington, D. C. (40).
Clark, John S., 646 Washington St., Boston, Mass. (31). I B C
Clark, Oliver Durfee, 248 Schenck St., Brooklyn, N. Y. (41). F E
Clark, Thomas H., Clark Univ., Worcester, Mass. (40).
Clark, Tracy Earl, B. S., Pembroke, Genesee Co., N. Y. (41). F
Clark, Wm. Brewster, M.D., 50 E. 31st St., New York, N. Y. (33). F C
Clarke, Francis Devereux, Principal Ark. Deaf Mute Inst., Little Rock, Arkansas (39). H
Clarke, Robert, Cincinnati, Ohio (30). H
Clarke, Sherman, 224 Alexander St., Rochester, N. Y. (41). C
Clendenin, Miss Ida May (40). F
COE, HENRY W., M.D., Oregonian Building, Portland, Oregon (32). H F
Coffin, Amory, Phœnixville, Chester Co., Pa. (31). D
Coggin, Wm. Thos., M D., M.A., Athens, Ga. (41). C F
Coit, J. Milner, Ph.D., Saint Paul's School, Concord, N. H. (33). B C E
COLBURN, RICHARD T., Elizabeth, N. J. (31). I F H
Cole, Aaron H., Greenwich, N. Y. (41). E F
Colie, Edw. M., East Orange, N. J. (30). E I
Collie, Prof. Geo. L., Beloit College, Beloit, Wis. (42). E
Collin, Rev. Henry P., Coldwater, Mich. (37). F
Collins, Prof. Jos. V., Miami Univ., Oxford, Ohio (37). A
Collins, William H., Haverford College, Haverford, Pa. (41). A
Colonna, B. A., U. S. C. and G. Survey, Washington, D. C. (37). E
Colton, Buel P., Normal, McLean Co., Ill. (34). F
Comstock, Dr. T. Griswold, 3401 Washington Ave., St. Louis, Mo. (29). F H
Conant, Miss E. Ida, 42 West 48th St., New York, N. Y. (33). H I F
Condit, Chas. L., 1323 G St., Washington, D. C. (40).
Conklin, Prof. Roland E., A.M., Eureka College, Eureka, Ill. (42). F
Conklin, W. A., Director Central Park Menagerie, New York, N. Y. (29). F
Cook, Dr. Charles D., 133 Pacific St., Brooklyn, N. Y. (25).
Coon, Henry C., M.D., Alfred Centre, N. Y. (29). B C F
Cope, Thos. P., Awbury, Germantown, Pa. (33). I
Cordley, Arthur B., Washington, D. C. (40).
Cowles, Alfred H., 656 Prospect St., Cleveland, Ohio (37). B C
Crafts, Robert H., 2329 So. 6th St., Minneapolis, Minn. (32). I B
Craig, John, Horticulturist, Experimental Farms, Ottawa, Ontario, Can. (41).
Craig, Oscar, Rochester, N. Y. (41). I H
Crandall, Prof. C. S., Fort Collins, Col. (40).
Crawford, John, Leon, Nicaragua, C. A. (40). E H

Crawley, J. T. (40).
Cresson, Dr. Hilborne T., 224 So. Broad St., Philadelphia, Pa. (39). **H**
Crockett, Charles W., Rensselaer Polytechnic Inst., Troy, N. Y. (39). **A D**
CROWELL, A. F., Woods Holl, Mass. (30). **C**
Cruikshank, James, LL.D., 206 So. Oxford St., Brooklyn, N. Y. (36).
Crump, M. H., Col. Commanding 3d Reg. K. S. G., Bowling Green, Ky. (29). **E**
Crump, Shelley G., Pittsford, N. Y. (41). **F**
Cummins, W. F., Dallas, Texas (37). **E**
Cunningham, Francis A., 1613 Wallace St., Philadelphia, Pa. (33). **D E B**
Cunningham, Prof. Susan J., Swarthmore College, Swarthmore, Pa. (38). **A**
Cuntz, Johannes H., 325 Hudson St., Hoboken, N. J. (36).
Curtis, Edw., M.D., 120 Broadway, New York, N. Y. (36).
Curtis, William E., Bureau of American Republics, State Department, Washington, D. C. (40). **H I**
Curtman, Dr. Charles O., 3718 North 9th St., St. Louis, Mo. (39). **C**
Cushing, Frank H., Bureau of Ethnology, Washington, D. C. (40). **H**
Cutler, Dr. Andrew S., Kankakee, Ill. (32). **I E**

Dains, Frank Burnett, Wesleyan Univ., Middletown, Conn. (41). **C**
DALY, HON. CHARLES P., 84 Clinton Place, New York, N. Y. (36).
Dana, James Jackson, Lt. Col. and Brevet Brig. Gen. U. S. Army, "Cosmos Club," 1520 H St., N. W., Washington, D. C. (40).
Daniel, John, Vanderbilt Univ., Nashville, Tenn. (38). **B**
Daniells, Prof. William W., Univ. of Wis., Madison, Wis. (42). **C**
Daniels, Edw. (32).
Davenport, Prof. Eugene, Woodland, Mich. (39).
Davidson, R. J., Experiment Station, Blacksburg, Va. (40). **C**
Davis, C. H., Commander U S. Navy, Chief Intelligence Officer, Navy Department, Washington, D. C. (40).
Davis, Dr. Charles H. S., Meriden, Conn. (40).
Davis, Prof. Floyd, Pres. New Mexico School of Mines, Socorro, N. M. (39). **C E**
Davis, J. C. Bancroft, 1621 H St., N. W., Washington, D. C. (40).
Davis, J. J., M.D., 1119 College Ave., Racine, Wis. (31). **F G**
Davison, John M., 60 Oxford St., Rochester, N. Y. (38). **C**
Dawson, Geo. M., D.S.C., F.G.S., Geol. Surv., Ottawa, Ontario, Can. (38).
Dean, Seth, Glenwood. Iowa (34). **D**
Decker, Edward P., Hospital for Insane, Evansville, Ind. (40).
DeCourcy, Bolton Waller, Ocosta, Washington (41). **I D**
DeForest, Henry S., Pres. Talladega College, Talladega, Alabama (32). **H I**
Deghnée, Joseph A., 247 Harrison St., Brooklyn, N. Y. (40). **C**
Densmore, Prof. H. D., Beloit, Wis. (41). **G**

Dewart, Frederick W., Missouri Botanical Garden, St. Louis, Mo. (41). F
Dewey, L. H., Dept. of Agric., Washington, D. C. (40) F
Dexter, Julius, Cincinnati, Ohio (30).
Dinsmore, Prof. Thos. H., jr., Emporia, Kan. (29). B C
Dinwiddie, William, Bureau of Ethnology, Washington, D. C. (40). H
Dittenhoefer, A. J., 96 Broadway, New York, N. Y. (36).
Dixwell, Epes S., Cambridge, Mass. (1). H F
Dodge, Charles Wright, M.S., Univ. of Rochester, Rochester, N. Y. (39). F
Dodge, Melvin Gilbert. Hamilton College, Clinton, N. Y. (42). C
Dodge, Wm. C., 116 B St., N. E., Washington, D. C. (40). H
Doolittle, Alfred, 910 G St., N.W., Washington, D. C. (40). A
Doolittle, Miss Mary A., 17 Grove Place, Rochester, N. Y. (41).
Doran, Edwin W., Ph.D., College Park, Md. (40) F I
Doubleday, H. H., 715 H St., N.W., Washington, D. C. (40). H
Doughty, John W., 165 Johnston St., Newburgh, N. Y. (19). E
Dow, Frank F., M.D., 60 South Ave., Rochester, N. Y. (41). F H
Dow, Mrs. Frederick C., North Elm St., Manchester, N. H. (42) E F G H
Dowling, Thomas, jr., 614 E St., N. W., Washington, D. C. (40). H
Drescher, Willibald A. E., P. O. Drawer 1033, Rochester, N. Y. (41). F
Drummond, Isaac Wyman, Ph.D., 436 W. 22nd St., New York, N.Y. (36).
Dryer, Chas. R., Fort Wayne, Ind. (38). E
Dudek, Miss Katie M., 54 W. 55th St., New York, N. Y. (36). E
Dulaney, Judge William L., Bowling Green, Ky. (39).
Dunham, Dr. Carroll, Irvington-on-Hudson, New York, N. Y. (31). F
Dunston, Robert Edw., Room 86, Foster Block, Hartford, Conn. (35). D
DuPont, Francis G., Wilmington, Del. (33). A B D
Du Pré. Prof. Daniel A., Wofford College, Spartanburg, S. C. (28). B C E
Durand. Elias J., Canandaigua, N. Y. (41). F
Durfee, W. F., Birdsboro, Berks Co., Pa. (33). D C B A E I
Dyer, Clarence M., Lawrence, Mass. (22).

Earle, F. S., Ocean Springs, Miss. (39). G
Eastman, Charles Rochester, Ass't Geologist, U. S. Geol. Survey, Cambridgeport, Mass. (41). E
Eccles, Robert G., M.D., 191 Dean St., Brooklyn, N. Y. (31). F C
Edelheim, Carl, 253–259 N. Broad St., Philadelphia, Pa. (33).
Edson, Hubert, Ass't Chemist Dept. of Agric., Washington, D. C. (40).
Edson, Joseph R., 1003 F St., N. W., Washington, D. C. (40). E F H
Edwards, J. W., P. O. Box 282, Rico, Col. (32).
Edwards, W. F., 48 E. Univ. Ave., Ann Arbor, Mich. (33). B C
Eichelberger, William Snyder, Ph.D., Wesleyan Univ., Middletown, Conn. (41). A
Ekeley, Prof. John B., The Cathedral School of Saint Paul, Garden City, L. I. (42). C

Elmer, Howard N., St. Paul, Minn. (32). D I
Emery, Frank E., No. Caro. Experiment Station, Agric. and Mechan. Coll., Raleigh, N. C. (38). F
Emmens, Stephen H., Youngwood, Westmoreland Co., Pa. (41).
English, Geo. L., 733 Broadway, New York, N. Y. (36).
ESTES, DANA, Brookline, Mass. (29). I
Estes, Ludovic, Grand Forks, No. Dakota (41). B
Evans, S. G., 211 Main St., Evansville, Ind. (39). F
Evers, Edw., M.D., 1861 North Market St., St. Louis, Mo. (28). F H
Ewell, Ervin E., Dept. of Agric., Chem. Division, Washington, D. C. (40). C
Ewell, Marshall D., M.D., Rooms 613 and 614, Ashland Block, 59 Clark St., Chicago, Ill. (40).

Fairbank, Miss Helen G., 1801 Michigan Ave., Chicago, Ill. (41). H
Fairchild, Benj. T., 82 Fulton St., New York, N. Y. (36).
Fairchild, Gen. Lucius, 302 Monroe St., Madison, Wis. (42). I
Fairfield, W. B., U. S. C. and G. Survey, Washington, D. C. (40). E
Falconer, Wm., Glen Cove, Queens Co., N. Y. (29).
Farnsworth, P. J., M.D., Clinton, Iowa (32). E H
Fellows, G. S., 1317 Q St., Washington, D. C. (36).
Ferree, Charles Maley, Kansas City, Mo. (37).
Ferry, Ervin S., Cornell Univ., Ithaca, N. Y. (41).
Fischer, E. G., U. S. Coast and Geodetic Survey, Washington, D. C. (40). A
Fisher, Miss Ellen F., Lake Erie Seminary, Painesville, Ohio (33). B A
Fisher, Geo. E., Rochester, N. Y. (37).
Fisher, Dr. R. Catlin, 1225 Conn. Ave., Washington, D. C. (36).
Fitz, Prof. Newton, Norfolk, Va. (30). A I
Flanders, Charles S., Franklin, Mass. (42). E
Flint, Weston, Statistician, U. S. Bureau of Education, 1101 K St., N. W., Washington, D. C. (40).
Foltz, Kent O., M.D., Akron, Ohio (36).
Ford, Prof. D. R., Elmira, N. Y. (41). A B
Ford, Mrs. O. M. (36).
Fortescue, Kenyon J., 57 Fifth Ave., New York, N. Y. (39). H I
Fortescue, Miss Maude, 57 Fifth Ave., New York, N. Y. (40). H
Forwood, Dr. W. H., Soldiers' Home, Washington, D. C. (40).
Foster, Prof. Eugene H., Shattuck School, Faribault, Minn. (39).
Francis, Prof. J. M., Tuscaloosa, Ala. (40). C
Freeman, Prof. T. J. A., Woodstock Coll., Howard Co., Md. (33). B C
Frick, Prof. John H., Central Wesleyan College, Warrenton, Mo. (27). E F B A
Frisbie, J. F., M.D., Box 455, Newton, Mass. (29). E H
Frost, Howard V., Ph.D., Arlington, Mass. (38). C
FROTHINGHAM, MRS. LOIS R., Milton, Mass. (31). F A I
Fuelling, J. L., Ass't Chem., Dept. of Agric., Washington, D. C. (40).

Fuller, Chas. G., M.D., Room 39. Central Music Hall, Chicago, Ill. (35). F
Fuller, Levi K., Brattleboro, Vt. (34). D A
Fuller, Melville W., LL.D., Chief Justice U. S., 1800 Mass. Ave., Washington, D. C. (40).

Gable, George D., Ph.D., Lafayette College, Easton, Pa. (40). A B
Gardner, Rev. Corliss B., 8 New York St., Rochester, N. Y. (29). A B I
GARLAND, JAMES, 2 Wall St., New York, N. Y. (36).
Garman, Harrison, Lexington, Ky. (38).
Garnett, Algernon S., M.D., Hot Springs, Ark. (23).
Garnier, Madame Laure Russell, 227 Columbus Ave., New York, N. Y. (40).
Gause, Frederick T., care Stevens Inst. of Tech., Hoboken, N. J. (40).
GENTH, FRED. A., Landsdowne, Del. Co., Pa. (32). C E
Genung, Nelson H., Ardmore, Pa. (40). B
Georgeson, Charles C., M.Sc., Manhattan, Kan. (42). I
Gere, Geo. W., Champaign, Ill. (40).
Ghequier, A. de, P. O. Box. 565, Washington, D. C. (30). I
Gibson, Chas. B. (34).
Gibson, J. Stewart, Montclair, N. J. (39). C B
Giddings, Frekerick S., Madison, Wis. (42). I
Gilbreth, Miss Mary E., 156 West Chester Park, Boston, Mass. (42). G
Gill, Adam Capen, Northampton, Mass. (38).
Glison, George Fredom, Pleasanton, Alameda Co., Cal. (41). H
GLENNY, WILLIAM H., JR., Buffalo, N. Y. (25).
Golden, Miss Katherine E., La Fayette, Ind. (42). G
Goler, George W., M D., 54 So. Fitzhugh St., Rochester, N. Y. (41). F
Goodale, Greenleaf A., Capt. 23rd Inf., U. S. A., Fort San Houston, San Antonio, Texas (39). H
Goodnow, Henry R., 32 Remsen St., Brooklyn, N. Y. (32). B
Gordon, Prof. Joseph C., National College for the Deaf, Kendall Green, Washington, D. C. (27).
Goss, Prof. Wm. F. M., La Fayette, Ind. (39).
Gould, Sylvester C., Manchester, N. H. (22). A B E H
Gould, Vernon, M.D., Rochester, Ind. (39).
Graef, Edw. L., 40 Court St., Brooklyn, N. Y. (28). F
Graf, Louis, Van Buren, Crawford Co., Ark. (30). E F H
Grant, H. L., 206 Moss Ave., Peoria, Ill. (39). C
Greely, Adolphus W., Signal Office, Washington, D. C. (39).
Green, Edgar Moore, M.D., Easton, Pa. (36).
Green, Milbrey, M.D., 567 Columbus Ave., Boston, Mass. (29).
Greene, Charles W., M.D., Merchantville, N. J. (41). E G H
Greene, Prof. Edward Lee, Univ. of Cal., Berkeley, Cal. (42). G
Greene, G. K., 195 West 1st St., New Albany, Ind. (38).
Greene, Jacob L., Pres. Mut. Life Ins. Co., Hartford, Conn. (28).
Greene, Jeannette B., M.D., Sci.D., F.E.C., 56 W. 55th St., New York, N. Y. (33). F E C

Greene, Thos. A., 297 East Water St., Milwaukee, Wis. (31). E
Greenleaf. R. P., M.D., 803 Market St., Wilmington, Del. (31). B F
Greenough, W. W., 299 Marlborough St., Boston, Mass. (29). D I
Gregory, Emily L., 343 Madison Ave., New York, N. Y. (41).
Greve, Theodor L. A., M.D., 260 W. 8th St., Cincinnati, Ohio (30).
Griscom, Wm. W., Haverford College P. O., Pa. (33). B C D
Gudeman, Edward, Ph.D., care Amer. Glucose Co., Buffalo, N. Y. (40). C
Gulliver, F. P., Norwich, Conn. (40). E
Gunckel, Lewis W., 121 West Second St., Dayton, Ohio (41). H
Gurley, Wm. F. E., Danville, Vermilion Co., Ill. (37). E

Hacker, William, 233 So. 4th St., Philadelphia, Pa. (33). F E
Hagemann, John, 125 Rusk St., Houston, Texas (29). C
Haight, Stephen S., C.E., 1266 Clover St., West Farms, New York, N. Y. (31). D
Hale, George D., 5 Gibbs St., Rochester, N. Y. (41).
Hale, William H., Ph.D., 40 First Place, Brooklyn, N. Y. (32). I F H C B E A
Hall, Arthur G., Grand Rapids, Mich. (41). A B
Hall, James P., 6 Poplar St., Brooklyn, N. Y. (40).
Hall, Stanton L., M.D., 250 Westchester Ave., Port Chester, N. Y. (36). H
Hall, Winfield S., M.D., Haverford, Pa. (40).
Hallock, Albert P., Ph.D., 440 First Ave., New York, N. Y. (31). C
Hammon, W. H., Weather Bureau, St. Louis, Mo. (37). B
Hammond, Geo. W., "The Hamilton," 260 Clarendon St., Boston, Mass. (28). C D
Hammond, Mrs. Geo. W., "The Hamilton," 260 Clarendon St., Boston, Mass. (29). H
Harmon, Miss A. Maria, 49 Daly Avenue, Ottawa, Ontario, Canada (31). H F
Harper, Prof. Charles A., Univ. of Cincinnati, Cincinnati, Ohio (40). C
Harrington, Prof. Mark W., Chief of Weather Bureau, Washington, D. C. (40). B
Harrington, W. H., Post Office Department, Ottawa, Ontario, Canada (29). F
Harris, A. W., Office Exper. Stations, Dept. of Agric., Washington, D. C. (40).
Harris, Gilbert D., Asst. Paleont., U. S. Geol. Survey, Smithsonian Institution, Washington, D. C. (37).
Harris, I. H., Waynesville, Warren Co., Ohio (30). E H
Harris, James Robert, Raleigh, N. C. (41). C
Harris, Mrs. Robert, Buckingham Hotel, New York, N. Y. (36).
Harrison, Caleb N., 1010 N. Arlington Ave., Baltimore, Md. (42). A B
Harrison, Miss Carrie, 1413 N St., N. W., Washington, D. C. (42). G

Harrison, Edwin, 520 Olive St., Room 620, St. Louis, Mo. (11). E
Harshmann, W. S., Nautical Almanac Office, Washington, D. C. (40).
Hart, C. Porter, M.D., Wyoming, Hamilton Co., Ohio (30). F
Hart, Rev. Prof. Samuel, Trinity College, Hartford, Conn. (22). A
Harvey, A. F., Kirkwood, Mo. (40).
Hasbrouck, Edwin M., 1610 15th St., Washington, D. C. (40).
Haskell, Eugene E., U. S. Engineer Office, Sault Ste. Marie, Mich. (39). A B D
Hasse, Hermann E., Santa Monica, Los Angeles Co., Cal. (33). F
Hasskarl, G. C. H., Ph.D., Frederick City, Md. (38).
Hatch, John W. (40).
Hathaway, Prof. A. S., Rose Polytechnic Inst., Terre Haute, Ind. (41). A
Hathaway, Nath'l, New Bedford, Mass. (30). C
Haven, Franklin, jr., New England Trust Co., Boston, Mass. (29).
Hay, Robert, Box 562, Junction City, Kan. (36). E
Hayden, Everett, Lt. U. S. N., Hydrographic Office, Navy Dept., Washington, D. C. (40).
Hayes, Charles Willard, U. S. Geol. Survey, Washington, D. C. (41). E
Hayes, Richard, 610 Olive St., St. Louis, Mo. (27). A B
Haywood, Prof. John, Otterbein Univ., Westerville, Ohio (30). A B
Hazen, Henry Allen, P. O. Box 427, Washington, D. C. (33). B
Head, W. R., 5467 Jefferson Ave., Hyde Park, Chicago, Ill. (38). E
Hedge, Fred. H., jr., Public Library, Lawrence, Mass. (28). F H
Hedges, Sidney M., 178 Devonshire St., Boston, Mass. (29).
Hedrick, Henry B., A.B., Nautical Almanac Office, Washington, D. C. (40).
Henderson, Mrs. Alice Palmer, 2301 Clinton Ave., Minneapolis, Minn (42). H
Henderson, C. Hanford, Manual Training School, Philadelphia, Pa. (33). E C B
Hendricks, Henry H., 49 Cliff St., New York, N. Y. (30).
Hershey, Oscar H., 38 American St., Freeport, Ill. (42). E
Hertzberg, Prof. Constantine, 181 S. Oxford St., Brooklyn, N. Y. (29). B F
Herty, Chas. Holmes, Ph.D., Univ. of Georgia, Athens, Ga. (42).
HEXAMER, C. JOHN, C.E., 419 Walnut St., Philadelphia, Pa. (33). C B
Heyer, Wm. D., 101 Pearl St., Elizabeth, N. J. (33). B D
Hice, Richard R., Beaver, Beaver Co., Pa. (41). E
Hicks, Geo. E., Great Neck, Long Island, N. Y. (36).
Hicks, John S., Roslyn, N. Y. (31). I
Hill, David J., Pres. Univ. of Rochester, Rochester, N. Y. (41). H I
Hill, Geo. Wm., Dept. of Agric., Washington, D. C. (40). I
Hillyer, Homer W., Ph.D., Univ. of Wis., Madison, Wis. (42). C
Hinds, Clara Bliss, M.D., 1331 N St., Washington, D. C. (40). H
Hinton, John H., M.D., 41 West 32nd St., New York, N. Y. (29). F H
Hinton, Richard J., Dept. of Agric., Washington, D. C. (40). I
Hitchcock, Miss Fanny R. M. (35). F
Hitchcock, Hiram, Fifth Ave. Hotel, New York, N. Y. (36).

Hoadley, Geo. A., A.M., Swarthmore College, Swarthmore, Pa. (40).
Hobbs, Prof. Perry L., Western Reserve Medical College, Cleveland, Ohio. (41). C
Hodge, J. M., Big Stone Gap, Va. (29). D E
Hodges, Julia, 139 W. 41st St., New York, N. Y. (36). E F H
Hodgkins, Prof. H. L., Columbian University, Washington, D. C. (40). A B
HOE, MRS. R., JR., 11 E. 36th St., New York, N. Y. (36).
Hoe, Mrs. Richard M., 1 E. 69th St., New York, N. Y. (36).
Hoeltge, Dr. A., 322 Lime St., Cincinnati, Ohio (30).
Hoffman, The Rev. Eugene Aug., D.D., Dean of Gen. Theol. Seminary, 426 W. 23d St., New York, N. Y. (36).
HOLDEN, MRS. L. E., The Hollenden, Cleveland, Ohio (35).
Holden, Perry G., Agricultural College, Michigan (41).
Holland, Rev. W. J., D.D., Ph.D., Pittsburg, Pa. (37). F
Holley, George W., Ithaca, N. Y. (19). B I
Hollingsworth, Jno. E., Austin, Texas (40).
Hollinshead, William H., Vanderbilt Univ., Nashville, Tenn. (37).
Holmes, W. Newton, Pritchett School Inst., Glasgow, Mo. (36).
Holstein, Geo. Wolf, Albany, Shackelford Co., Texas (28). E H
Holt, Henry, 29 West 23d St., New York, N. Y. (29).
Homburg, Frederick, 40 Clifton Ave., Cincinnati, Ohio (39). C
Homer, Chas. S., jr., of Valentine & Co., 245 Broadway, New York, N. Y., (29).
Hood, E. Lyman, Albuquerque, N. M. (30). F I
Hood, Gilbert E., Lawrence, Mass. (29). H E B
Hood, William, 512 Van Ness Ave., San Francisco, Cal. (35). D
Hooper, Dr. F. H., 460 County, cor. William St., New Bedford, Mass. (29).
Hooper, Josephus, M.D., Louisville, Ky. (39).
Hoover, Prof. William, Box 248, Athens, Ohio (34). A
Hopkins, A. D., Exper. Station, Morgantown, W. Va. (42). F
Hopkins, Mrs. Alice L., 17 Grove Place, Rochester, N. Y. (41).
Hopkins, Grant S., Ithaca, N. Y. (41). F
Hopkins, Thos. C., Dept. of Geol., Chicago Univ., Chicago, Ill. (38). E
Horr, Asa, M.D., 1311 Main St., Dubuque, Iowa (21). B E
Horton, Horace E. L., Ass't Chem. Dept. of Agric., Washington, D. C. (40).
Hoskins, William, La Grange, Cook Co., Ill. (34). C
Hough, Romeyn B., Lowville, N. Y. (37).
Hovey, Edmund O., New Haven, Conn. (36). C E
Howell, David J., 939 F St., N. W., Washington, D. C. (40). E
Hoyt, James T., Temple Court, Beekman St., New York, N. Y. (38). A H
Hubbard, Gardiner Greene, 1328 Conn. Ave., Washington, D. C. (40). E
Hubbard, George W., M.D., Nashville, Tenn. (26). F
Hubbard, Henry Guernsey, 114 Griswold St., Detroit, Mich. (41).
Hudson, George H., Plattsburgh, Clinton Co., N. Y. (31). F
Hugo, T. W., Duluth, Minn. (33). D

Hume, Alfred, C. E., University, Miss. (39). A
Humphrey, Daniel, M.D., Lawrence, Mass. (18). F H
Humphreys, William J., Washington College, Chestertown, Md. (42) B
Hunt, Richard M., Metropolitan Building, 1 Madison Ave., New York, N. Y. (36).
Hunt, Miss Sarah E., Salem, Mass. (20).
Hunter, Andrew Frederick, Barrie, Ontario, Can. (38). B H I
Huntington, Elon, 762 N. St. Paul St., Rochester, N. Y. (41). B E
Hurd, E. O., Plainville, Hamilton Co., Ohio (30). E F
Hussey, Wm. J., Leland Stanford, jr. Univ., Palo Alto, Cal. (39). A
Huston, Henry A., LaFayette, Ind. (37). C
Hutchinson, Wm. M., M.D., 207 Clinton St., Brooklyn, N. Y. (40). C B

Iles, George, 7 Brunswick St., Montreal, Can. (31). I
Ingalls, Jas. M., Capt. 1st Art'y, U. S. A., Fortress Monroe, Va. (35).
Ingham, Wm. A., 320 Walnut St., Philadelphia, Pa. (33). E I

Jackson, Prof. Josiah, State College, Centre Co., Pa. (35). A
James, Bushrod W., M.D., N. E. cor. 18th and Green Sts., Philadelphia, Pa. (29). F
James, Davis L., 181 West 7th St., Cincinnati, Ohio (30). F
James, John N., U. S. Naval Observ., Washington, D. C. (40). B
Janney, Reynold, Chillicothe, Ohio (30). B A
Jefferis, Wm. W., 1836 Green St., Philadelphia, Pa. (33). E
Jenks, Wm. H., Brookville, Pa. (38).
Jenner, Charles H., Prof. of Natural and Applied Sciences, Brockport, N. Y. (41). A B D
Jennings, Mrs. N. B., 140 Plymouth Ave., Rochester, N. Y. (41).
Jennings, W. T., Consulting Eng., Room 506, Board of Trade Building, Toronto, Ontario, Canada (38).
Jesunofsky, Lewis N., U. S. Weather Bureau, Charleston, S. C. (36). B
Jewett, Franklin N., Fredonia, N. Y. (41). E H
Johnson, Henry Clark, Pres. Central High School, Philadelphia, Pa. (42). I
Johnson, Dr. Henry L. E., 1400 L St., N. W., Washington, D. C. (40). F
Johnson, Lorenzo N., 47 South University Ave., Ann Arbor, Mich. (39). F
Johnson, Nels, Manistee, Mich. (41). A B
Jones, Prof. Forrest R., Univ. of Wisconsin, Madison, Wis. (42). D
Jones, Lewis R., Burlington, Vt. (41). G
Jones, Paul M., D.Sc., Nashville, Tenn. (40). E F
Jones, William S., 109 Huron St., Cleveland, Ohio (37).

Karslake, William J., Le Roy, Genesee Co., N. Y. (41).
Kedzie, John H., Evanston, Ill. (34). B
Keep, Wm. J., Detroit, Mich. (37).
Keffer, Frederic, 64 West 9th Ave., Columbus, Ohio (37).

Kelley, Henry S., 208 Wooster St., New Haven, Conn. (36). D C
Kellogg, David S., M.D., Plattsburgh, N. Y. (29). H
Kellogg, George, 112 E. 40th St., New York, N. Y. (36)
Kellogg, John H., M.D., Battle Creek, Mich. (24). F
Kemper, Dr. And. C., 101 Broadway, Cincinnati, Ohio (30). F H C E I D B A
Kennedy, Dr. George Golding, Roxbury, Mass. (40). F
Kennedy, Prof. George T., Kings College, Windsor, N. S. (29). E C
KENNEDY, HARRIS, 284 Warren St., Roxbury, Mass. (40). E F
Kern, Josiah Quincy, Ph.D., Treasury Dept., Washington, D. C. (40). I
Kilborne, Dr. Fred L., Dept. of Agric., Washington, D. C. (40).
Kinder, Miss Sarah A., 28 Lockerbie St., Indianapolis, Ind. (39).
King, A. F. A., M.D., 1315 Mass. Ave., N. W., Washington, D. C. (29). F H
King, Miss Ada M., 8 Briggs Place, Rochester, N. Y. (39). E I
King, Charles F., Steelton, Pa. (33). C E
King, Miss Harriet M., Salem, Mass. (28).
King, Wm. R., Director Honduras Botanical Gardens, Patuca, Republic of Honduras, "via Truxillo" (40). G
Kinner, Hugo, M.D., 1517 South Seventh St., St. Louis, Mo. (21). F H
Kinyoun, Joseph J., M.D., U. S. Marine Hospital Service, Washington, D. C. (40). F
Kirk, Hyland C. (40). B F H
Kirkpatrick, Walter G., West End Ave., Nashville, Tenn. (41).
Kittredge, Miss H. A., North Andover, Mass. (37). F
Klie, G. H. Carl, 5100 No. Broadway, St. Louis, Mo. (39). C F
Knickerbacker, John, C.E., Troy, N. Y. (36).
Knight, Albert B., P. O. Box 211, Butte City, Silver Bow Co., Montana (36). D
Knight, Chas. H., M.D., 20 W. 31st St., New York, N. Y. (36).
Knight, Prof. Charles M., 219 So. Union St., Akron, Ohio (29). C B
Knox, Wilm, care C. O. Child, Painesville, Ohio (38).
Kober, Geo. Martin, M.D., 1819 Q St., N. W., Washington, D. C. (40). H
Kocherspergee, Miss Nellie, 934 Kurtz St., Philadelphia, Pa. (40).
Kohler, Elmer P., Ph.D., Egypt, Lehigh Co., Pa. (41). C
Kost, John, LL.D., Adrian, Mich. (34). E
Koues, Miss Elizabeth L., 10 E. 75th St., New York, N. Y. (41). I
Krécsy, Prof. Béla, Royal States High School, Kecskemét, Hungary (41). C
Krug, Wm. H., Ass't Chem. Dept. of Agric., Washington, D. C. (40).
Kubel, S. J., U. S. Geol. Survey, Washington, D. C. (40).
Kuhne, F. W., 19 Court St., Fort Wayne, Ind. (38). A F

Ladd, G. E., Bradford, Mass. (39). E
Lamb, Daniel S., M.D., 800 10th St., N. W., Washington, D. C. (40). H
Lambert, Preston A., 422 Walnut St., South Bethlehem, Pa. (41). A

Lamborn, Robert H., Ph.D., 32 Nassau St., New York, N. Y. (28). H E F
Lampard, Henry, 102 Shuter St., Montreal, Can. (40).
LANDERO, CARLOS F. DE, Ass't Director, Pachuca and Real del Monte Mining Co., Pachuca, Mexico. (36). C B
Langenbeck, Karl, 27 Orchard St., Zanesville, Ohio (39). C
Lang, Prof. Henry R., Yale Univ., New Haven, Conn. (41). H
Langmann, Gustav, M.D., 115 W. 57th St., New York, N. Y. (36).
Lasché, Alfred, 688 W. Chicago Ave., Chicago, Ill. (39). C F G
Latham, Miss Vida Annette, D.D.S., F.R.M.S., Northwest Univ., Woman's Medical School, 333 S. Lincoln St., Chicago, Ill. (39).
Latta, Prof. William C., LaFayette, Ind. (37).
Lawrance, J. P. S., Past Ass't Engineer, U. S. N., Navy Yard, Norfolk, Va. (35). D
Laws, Miss Annie, 100 Dayton St., Cincinnati, Ohio (30). I
Leach, Hamilton E., M.D., 716 13th St., N.W., Washington, D. C. (40).
Ledyard, T. D., 57 Colborne St., Room 3, Toronto, Ontario, Can. (38).
Lee, Mrs. William, care Lee & Shepard, 10 Milk St., Boston, Mass. (36).
Leeds, James S., 109 Produce Exchange, New York, N. Y. (41).
Leete, James M., M.D., 2912 Washington Ave., St. Louis, Mo. (27).
Lefavour, Prof. Henry, Williams College, Williamstown, Mass. (42).
Leiter, L. Z., 1500 20th St., Washington, D. C. (40).
Lemp, William J., cor. Cherokee and 2nd Carondelet Avenue, St. Louis, Mo. (27).
Lennon, William H., Brockport, N. Y. (31). G C
Leonard, Miss Georgia L., "The Ardmore," Atlanta, Ga. (40). H
Leoser, Charles McK., 34 Beaver St., New York, N. Y. (32). A
Leslie, Geo. L., Box 515, Santa Barbara, Cal. (40).
Letchworth, Josiah, Buffalo, N. Y. (25).
Lewis, Elias, jr., 111 St. Mark's Ave., Brooklyn, N. Y. (28). E H
Lewis, John E., Ansonia, Conn. (40). A B E
Lewis, Wm. J., M.D., 102 W. 75th St., New York, N. Y. (33). F E
Liebig, Dr. G. A., 26 South St., Baltimore, Md. (30).
Lilley, Geo., LL.D., Pullman, Washington (40). A I
Lincoln, Prof. David F., M.D., Hobart College, Geneva, N. Y. (41).
Lincoln, Nathan S., M.D., 1514 H St., N.W., Washington, D. C. (40).
Lindenkohl, Adolphus, U. S. Coast and Geodetic Survey, Washington, D. C. (40). E
Lindsay, Alexander M., Rochester, N. Y. (41).
Lindsay, Prof. Wm. B., Dickinson College, Carlisle, Pa. (41). C
Line, J. Edw., D.D.S., 50 Rowley St., Rochester, N. Y. (39). F
Linebarger, Charles E., Evanston, Ill. (41).
Livermore, Mrs. M. A. C., 24 North Avenue, Cambridge, Mass. (29). F
Livermore, Wm. R., Maj. of Eng., U. S. A., P. O. Building, Boston, Mass. (38). C
Locke, James, Buffalo, N. Y. (41). C
Loewy, Benno, 206 and 208 Broadway, New York, N. Y. (41).

Logan, F. G., 2919 Prairie Ave., Chicago, Ill. (42). H
Logan, Walter S., 58 William St., New York, N. Y. (36).
Lomb, Adolph, P. O. Drawer 1033, Rochester, N. Y. (41).
Lomb, Carl F., 543 No. St. Paul St., Rochester, N. Y. (29).
Lomb, Henry, P. O. Drawer 1033, Rochester, N. Y. (41).
Long, Prof. John H., 40 Dearborn St., Chicago, Ill. (41).
Lonsdale, Elston H., Ass't Missouri Geol. Survey, Jefferson City, Mo. (41). E
Loomis, Prof. Horatio, 43 Williams St., Burlington, Vt. (31).
Lord, Benjamin, 34 W. 28th St., New York, N. Y. (36).
Lowell, Aug., 60 State St., Boston, Mass. (29).
Lowell, Percival, 53 State St., Boston, Mass. (36). A
Lowman, John H., M.D., 345 Prospect St., Cleveland, Ohio (37).
Lowman, Oscar, Ph.D., 185 Jefferson Ave., Detroit, Mich. (39). C
Ludlow, Wm., Bv't Lt. Col. U. S A., U. S. Light House Eng., 9th and 11th Districts, Detroit, Mich. (33). D B
Lufkin, Albert, Newton, Iowa (31). D E
Lummis, Wm., 8 Broad St., Drexel Building, New York, N. Y. (36).
Lyford, Edwin F., Springfield, Mass. (33). B C H
LYMAN, BENJ. SMITH, 708 Locust St., Philadelphia, Pa. (15). E
Lyman, Henry H., 74 McTavish St., Montreal, P. Q., Can. (29). F E I
Lyon, Edmund, 110 So. Fitzhugh St., Rochester, N. Y. (41).

McAndrew, George J., Plattsburgh, N. Y. (40). H
MacArthur, Charles L., Troy, N. Y. (39).
McCammon, Gen. Joseph K., 1420 F St., Washington, D. C. (40).
McCarthy, Gerald, N. C. Agric. Exper. Station, Raleigh, N. C. (41).
McCartney, Dr. James H., 138 East Main St., Rochester, N. Y. (41). B
McClintock, A. H., Wilkes Barre, Pa. (33). H
McCorkle, Spencer C., U. S. C. and G. Survey Office, Washington, D. C. (33). A E
McCowen, Dr. Jennie, Davenport, Iowa (39).
McCulloch, C. C., Ph.D., M.D., Waco, Texas (39). E
McCurdy, Chas. W., Sc.D., Winona, Minn. (35). F E
McElroy, K. P., 2nd Ass't Chem. Div., Dept. of Agric., Washington, D. C. (40).
McFadden, Prof. L. H., Westerville, Ohio (32). B C
McFarland, Robert W., LL.D., Oxford, Ohio (33). A
McGee, Miss Emma R., Farley, Iowa (33). H
McGee, W. L., Agricultural College, Miss. (40). F
McHenry, Prof. B. F., Union Christian College, Merom, Ind. (39). A
McKeever, Chauncey, Brig. Gen. U. S. Army, Adj. General's Office, Washington, D. C. (40).
McLean, T. C., Lieut. U. S. N., New Hartford, Oneida Co., N. Y. (33).
Mac Millan, Prof. Conway, Univ. of Minnesota, Minneapolis, Minn. (42). G
McMillan, Smith B., Signal, Columbiana Co., Ohio (37).

McMillin, Emerson, Columbus, Ohio (37).
McWhorter, Tyler, Aledo, Ill. (20). E
Macdougall, Alan, 30 East Adelaide St., Toronto, Ontario, Can. (38 D H
Macfarlane, Dr. John M., Lansdowne, Del. Co., Pa. (41). F
Magruder, Wm. T., Vanderbilt Univ., Nashville, Tenn. (37).
Mallinckrodt, Edw., P. O. Sub-station A, St. Louis, Mo. (29). C
Mallory, Maitland L., M.D., 69 North Fitzhugh St., Rochester, N. Y. (39). F
Mann, Abram S., Rochester, N. Y. (39). E
Manning, Charles H., U. S. N., Manchester, N. H. (35). D
Manning, Miss Sara M., Lake City, Minn. (33). F
Manning, Warren H., Brookline, Mass. (31). F H E
Mapes, Charles Victor, 60 W. 40th St., New York, N. Y. (37). C
MARBLE, MANTON, 532 Fifth Ave., New York, N. Y. (36).
Marble, J. Russel, Worcester, Mass. (31). C E
Marble, Miss Sarah, Woonsocket, R. I. (29). C
Marbut, Curtis Fletcher, Ass't Missouri Geol. Survey, Jefferson City, Mo. (41). E
Marindin, Henry Louis, U. S. Coast and Geodetic Survey, Washington, D. C. (40). E
Markley, Joseph L., Ph.D., 50 Thompson St., Ann Arbor, Mich. (40).
Marlatt, Charles L. (40). F
Marmion, William Vincent, M.D., 1108 F St., N.W., Washington, D. C. (40).
Marple, Charles A., 717 W. Chestnut St., Louisville, Ky. (39). B
Marsden, Samuel, 1015 North Leffenwell Ave., St. Louis, Mo. (27). A D
Marvin, Frank O., Univ. of Kansas, Lawrence, Kansas (35). D
Mateer, Horace N., M.D., Wooster, Wayne Co., Ohio (36). F E
Mathews, Miss Mary Elizabeth, Lake Erie Seminary, Painesville, Ohio (41). F
Matlack, Charles, 924 N. 41st St., Philadelphia, Pa. (27). I
Mattison, Joseph G., 20 West 14th St., New York, N. Y. (30). C
May, John J., Box 2348, Boston, Mass. (29). D I
Maynard, Geo. C., 1227 19th St., Washington, D. C. (35). B D
Maynard, Prof. Samuel T., Agricultural College, Amherst, Mass. (38).
Maynard, Washburn, Lieut. Com'd U. S. N., Bureau of Ordnance, Navy Dep't, Washington, D. C. (33). B
Means, John H., Palo Alto, Cal. (38). E
Meeds, Alonzo D., Univ. of Minnesota, Minneapolis, Minn. (42).
Meehan, Mrs. Thos., Germantown, Pa. (29).
Mell, Prof. P. H., Polytechnic Inst., Auburn, Ala. (39). E
Mellor, Chas. C., 77 Fifth Ave., Pittsburgh, Pa. (38).
Merrick, Hon. Edwin T., P. O. Box 606, New Orleans, La. (29). E A
Merrill, Mrs. Winifred Edgerton, Ph.B., 2 Sprague Place, Albany, N. Y. (35). A

Merryweather, George N., cor. 6th and Race Sts., Cincinnati, Ohio (30). F H
Merwin, Orange, Bridgeport, Conn. (38). E
METCALF, ORLANDO, 424 Telephone Building, Pittsburgh, Pa. (35). D
Metcalf, William, Pittsburgh, Pa. (33).
Miller, Clifford N., 604 Greenup St., Covington, Ky. (37). D
MILLER, EDGAR G., 213 E. German St., Baltimore, Md. (29). E F A
Miller, John A., 2500 Park Ave., Cairo, Ill. (22). D
Miller, William S., M.D., 923 West Johnson St., Madison, Wis. (42).
Mills, Andrew G., Cotton Exchange, Galveston, Texas (33). I
Mills, James, M.A., Guelph, Ontario, Can. (31). I C
Mindeleff, Cosmos, Bureau of Ethnology, Washington, D. C. (38). H
Mindeleff, Mrs. Julie, 1401 Stoughton St., Washington, D. C. (40).
Minns, Miss S., 14 Louisburg Square, Boston, Mass. (32).
Mitting, Ebenezer K., 423 Superior St., Chicago, Ill. (40).
Mixer, Fred. K., 427 Delaware Ave., Buffalo, N. Y. (35). E
Mohr, Dr. Charles, Mobile, Ala. (40). G
Molson, John H. R., Montreal, P. Q., Can. (31).
Moody, Mrs. Mary B., M.D., Fair Haven Heights, New Haven, Conn. (25). E F
Moore, Burton E., 503 W. 4th St., South Bethlehem, Pa. (41).
Moore, Geo. D., Ph.D., Polytechnic Inst., Worcester, Mass. (40).
Moore, Joseph, LL.D., Earlham College, Richmond, Ind. (39).
Moran, Jno. F., M.D., 2420 Pennsylvania Ave., N. W., Washington, D. C. (40).
Moreland, Prof. S. T., Lexington, Va. (33). B D
Morey, Prof. William C., Rochester, N. Y. (41). H I
Morgan, Wm. F., Short Hills, N. J. (27).
Morrill, Prof. A. D., Hamilton College, Clinton, Oneida Co., N. Y. (37).
Morse, Mrs. Mary J., 57 Jackson St., Lawrence, Mass. (29). C
Mortimer, Capt. John H., care of Alex. Campbell & Co., 26 Pine St., New York, N. Y. (31).
Moseley, Edwin L., A. M., High School, Sandusky, Ohio (34).
Moss, Mrs. J. Osborne, Sandusky, Ohio (35). F
Mowry, Wm. A., 97 Federal St., Salem, Mass. (29). I
Muir, John, Martinez, Cal. (22).
Munson, Prof. Welton M., Maine State College, Orono, Me (41).
Murphy, Edward, M.D., New Harmony, Ind. (39). C

Neff, Peter, 361 Russell Ave., Cleveland, Ohio (37). E
Neff, Peter, jr., 361 Russell Ave., Cleveland, Ohio (34). B
Nelson, William, Rooms 7 and 8, Paterson National Bank, Paterson, N. J. (42).
Nelson, Wolfred, C.M., M.D., Astor House, New York, N. Y. (35). H E
Nesmith, Henry E., jr., 28 South St., New York, N. Y. (30). B F C
Neumoegen, Berthold, Box 2581, New York, N. Y. (40). F
Newell, F. H., U. S. Geol. Survey, Washington, D. C. (40).

Newell, William Wells, Sec'y Am. Folk Lore Society and Editor of the Journal, 175 Brattle St., Cambridge, Mass. (41). H
Nichols, A. B., Rosemont, Pa. (33). D
Nichols, Austin P., 4 Highland Ave., Haverhill, Mass. (37).
Northrop, Dr. Katharine, 309 S. 15th St., Philadelphia, Pa. (35). F
Norton, Prof. Wm. H., Mt. Vernon, Iowa (39). E
Nunn, R. J., M.D., 119 York St., Savannah, Ga. (33).

O'Connor, Joseph, 146 Frank St., Rochester, N. Y. (41).
O'Connor, Thomas Devlin, 12 E. 44th St., New York, N. Y. (36).
Olds, Prof. George D., Amherst, Mass. (38). A
Orleman, Miss Daisy M., M.D., 1517 Kingman Place, Washington, D. C. (40). B C F
Orr, William, jr., 133 Catharine St., Springfield, Mass. (39). F B
Osborne, Mrs. Ada M., Waterville, Oneida Co., N. Y. (19). E
Osborne, Amos O., Waterville, Oneida Co., N. Y. (19). E
Osgood, Joseph B. F., Salem, Mass. (31).
O'Sullivan, Rev. Denis T., S.J., Woodstock, Howard Co., Md. (40). B A
Oviatt, David B., Georgia School of Technology, Atlanta, Ga. (40). D
Owen, Prof. D. A., Franklin, Ind. (34). E

Page, Dr. D. L., 46 Merrimack St., Lowell, Mass. (33). F
Paine, Sidney B., Edison Electric Light Co., Boston, Mass. (30). D B
Palmer, Dr. Edward, care F. V. Coville, Dep't of Agric., Washington, D. C. (22). H
Pardo, Carlos, 150 Fifth Ave., New York, N. Y. (36). A
Parker, J. D., Chaplain, San Diego, Cal. (34). H
Parks, C. Wellman, U. S. Dept. of Education, Washington, D. C. (42).
Parks, Prof. R. M., Bedford, Ind. (39). C
Parks, Prof. William B., Thorp's Spring, Hood Co., Texas (40). E F
Parmelee, H. P., Hillsdale, Mich. (42). H E
Parsons, Prof. C. Lathrop, Hanover, N. H. (41).
PARSONS, JNO. E. (36).
Patton, Horace B., Golden, Col. (37). E
Paul, Caroline A., M.D., Vineland, Cumberland Co., N. J. (23).
Payne, Frank Fitz, Meteorological Office, Toronto, Ontario, Can. (38).
Peale, Albert C., M.D., U. S. Geol. Survey, Washington, D. C. (36). E
Pearce, James H., 226 Beverly St., Toronto, Ontario, Can. (38). F
Peck, Mrs. Emma J., 185 W. Chester Park, Boston, Mass. (40).
Peck, Mrs. John H., 3 Irving Place, Troy, N. Y. (28).
Peck, W. A., C.E., 1051 Clarkson St., Denver, Col. (19). E
Peckham, Wheeler H., Drexel Building, Wall St., New York, N. Y. (36).
Peffer, George P., Box 33, Pewaukee, Wis. (32). D I
Peirce, Cyrus N., D.D.S., 1415 Walnut St., Philadelphia, Pa. (31). F
Peirce, Harold, 331 Walnut St., Philadelphia, Pa. (33). H I
Pell, Alfred, Highland Falls, N. Y. (36).

Pendleton, Dr. Hunter, Lexington, Va. (40).
PERKINS, ARTHUR, 14 State St., Hartford, Conn. (31). B A
Perrin, John, care W. McGibbon, Mount Royal Park, Montreal, P. Q. (38).
Peters, Mrs. Bernard, 83 Lee Ave., Brooklyn, N. Y. (36).
Petitdidier, O. L., Mt. Carmel, Ill. (39). A B D
Pettee, Rev. J. T., Meriden, Conn. (39).
Pfeiffer, Prof. Geo. B., Washington High School, Westwood, Prince George Co., Md. (40). B C
Phillips, Dr. Wm. A., Evanston, Ill. (41). H
Phillips, W. Hallett, 603 Louisiana Ave., Washington, D. C. (40). H
Pickett, Dr. Thos. E., Maysville, Mason Co., Ky. (25). H F
Pickett, W. D., Arland, Big Horn Co., Wyo. (41). D I
Pierce, Josiah, jr., 806 17th St., Washington, D. C. (40) E
Pierce, Perry Benj., U. S. Patent Office, Washington, D. C. (40). H
Pike, J. W., Mahoning, Portage Co., Ohio (29). E C F
Pilling, J. W., 1301 Mass. Ave., Washington, D. C. (40).
Pillsbury, J. E., Lieut. U. S. N., 225 Commonwealth Ave., Boston, Mass. (33). E B
Pinkerton, T. H., M.D., P. O. Box 11, Oakland, Alameda Co., Cal. (27).
Place, Edwin, Terre Haute, Ind. (33). B
Pope, Edward S., 235 Blackford St., Indianapolis, Ind. (39).
Porteous, John, 176 Falmouth St., Boston, Mass. (22).
Porter, Edna, 77 Bryant St., Buffalo, N. Y. (41). F G
Post, Prof. Charles M., Alfred Centre, N. Y. (39). B
Poteat, Prof. William L., Wake Forest, N. C. (40). F
Potter, Mrs. Charles B., 111 Spring St., Rochester, N. Y. (41). H
Potter, Rev. Henry C., 804 Broadway, New York, N. Y. (29).
Potter, Henry Noel, 111 Spring St., Rochester, N. Y. (41). B
POTTER, O. B., 26 Lafayette Place, New York, N. Y. (36).
Powell, Thomas, 16 S. Main St., Fort Scott, Kansas (41).
Prang, Louis, 45 Centre St., Roxbury, Mass. (29). D
Preswick, E. H., Ithaca, N. Y. (35). C
Price, J. Sergeant, 709 Walnut St., Philadelphia, Pa. (33).
Priest, Geo. A., Census Office, Washington, D. C. (40).
Prince, Gen. Henry, U. S. A., Fitchburg, Mass. (22).
Prosser, Col. Wm. F., North Yakima, Yakima Co., Washington (26). E I
PRUYN, JOHN V. L., JR., Albany, N. Y. (29).
Pulsifer, Mrs. C. L. B., Newton Centre, Mass. (33).
Purinton, Prof. George D., Columbia, Mo. (31). C F
Putnam, Chas. P., M.D., 63 Marlborough St., Boston, Mass. (28).

Quick, Prof. Walter J., Fort Collins, Col. (40).
Quinche, Miss Helen M., Columbus, Miss. (40). C

Raenber, Edward G. (39). C F
Rand, C. F., M.D., 1228 15th St. N. W., Washington, D. C. (27). E H

Randolph, L. S., Engineer of Tests, B. & O. R. R. Co., Baltimore, Md (33). D
Rane, Frank Wm., Exper. Station, Morgantown, W. Va. (42).
Ray, P. H., Capt. U. S. Army, Omaha, Nebraska (40).
Read, Edmund E., jr., 604 Cooper St., Camden, N. J. (39). A B
Reber, Prof. Louis E., State College, Centre Co., Pa. (35). D
Rèche, Miss Eugénie M., 31 Howell St., Rochester, N. Y. (41). E H
Redding, Prof. Allen C., 1000 No. Cory St., Findlay, Ohio (39). C
Reed, Charles J., 609 Norris St., Philadelphia, Pa. (34). C B
Reed, Taylor, Princeton, N. J. (38). A
Reichel, Rev. George V., Brockport, N. Y. (41). F H
Remington, Cyrus K., 11 E. Seneca St., Buffalo, N. Y. (35). E
Renninger, John S., M.D., Marshall, Minn. (31). C F
Reyburn, Robert, M.D., 2129 F St., N. W., Washington, D. C. (33). F
Reynolds, Sheldon, Wilkes Barre, Pa. (33). H
Rich, Jacob Monroe, 50 W. 38th St., New York, N. Y. (33). B A
Rich, Michael P., 50 W. 38th St., New York, N. Y. (40).
Richmond, Geo. B., 213 Washington Ave., N. Lansing, Mich. (34). C B
Ricketts, Col. R. Bruce, Wilkes Barre, Pa. (33). E
Rideout, Bates S., Norway, Me. (31). E H
Ries, Elias E., 430 South Broadway, Baltimore, Md. (33). B I D
Ries, Heinrich, Ph.B., 102 W. 61st St., New York, N. Y. (41). E
Riggs, Chauncey Wales, care of H. C. Warinner, 14 Madison St., Memphis, Tenn. (41). H
Riggs, Geo. W., Summit, N. J. (26). C
Riggs, Lawrason, 814 Cathedral St., Baltimore, Md. (36).
Ritter, Homer P., U. S. C. and G. Survey, Washington, D. C. (40).
RIVERA, JOSÉ DE (29).
Robbins, A. J., M.D., Washington, D. C. (40).
Robbins, E. P. (30). D B
Robertson, Charles, Carlinville, Ill. (39). F
Robertson, James D., Ass't Missouri Geol. Survey, Jefferson City, Mo. (41). E
ROBERTSON, THOMAS D., Rockford, Ill. (10). E H
Robinson, Prof. Norman, Tallahassee, Florida (40).
Robinson, Prof. Otis Hall, 273 Alexander St., Rochester, N. Y. (23). B A
Rochester, DeLancey, M.D., 469 Franklin St., Buffalo, N. Y. (35). F
Rockwood, Charles G., Newark, N. J. (36).
Roessler, Franz, 73 Pine St., New York, N. Y. (39).
Rogers, Frederick J., Ithaca, N. Y. (40).
Rolfe, Charles W., Univ. of Illinois, Champaign, Ill. (32).
Rolfs, P. H., Florida Agricultural College, Lake City, Fla. (41).
Roosevelt, Hon. Robert B., 33 Nassau St., New York, N. Y. (33). B F
ROOSEVELT, MRS. MARION T., 57 Fifth Ave., New York, N. Y. (31). H I
Rose, Jos. N., Ass't Botanist, Dept. of Agric., Washington, D. C. (40).
Rosell, Claude A. O., 1131 9th St., N. W., Washington, D. C. (40).
Ross, Denman Waldo, Ph.D., Cambridge, Mass. (29).

Ross, Prof. Edward A., Cornell Univ., Ithaca, N. Y. (41). **I**
Rotch, A. Lawrence, Readville, Mass. (39).
Roth, Filibert, U. S. Dep't of Agric., Washington, D. C. (39). **F**
Rowell, Chas. E., M.D., Stamford, Conn. (33). **F H**
Rowlee, W. W., Cornell Univ., Ithaca, N. Y. (41). **G**
Runyan, Elmer G., Dept. of Agriculture, Washington, D. C. (40). **C**
Rupp, August, A.B., College of City of New York, New York, N. Y. (35).
Rupp, Philip, M.D., 84 Second Ave., New York, N. Y. (35).
Russell, A. H., Captain of Ordnance, U. S. A., care Chief of Ordnance, Washington, D. C. (38). **D**
Russell, H. L., Ph.D., Poynette, Wis. (41).
Rust, Horatio N., Colton, San Bernardino Co., Cal. (26). **H**
Ryder, John A., care Univ. of Pennsylvania, Philadelphia, Pa. (33).
Ryker, J. N., U. S. Weather Bureau, Lynchburg, Va. (41).

Sackett, Miss Eliza D., Cranford, N. J. (35). **F H**
Sage, John H., Portland, Conn. (23). **F**
Sander, Dr. Enno, St. Louis, Mo. (27). **C**
Sargent, Erle Hoxsie, Medina, Ohio (37). **F**
Saunders, Prof. Charles E., Experimental Farm, Ottawa, Can. (41). **C**
Saville, James H., Attorney-at-Law, 1419 F St., N. W., Washington, D. C. (29).
Sayre, Robert H., South Bethlehem, Pa. (28). **D**
SCHAFFER, CHAS., M.D., 1309 Arch St., Philadelphia, Pa. (29). **F E**
SCHAFFER, MRS. MARY TOWNSEND SHARPLESS, 1309 Arch St., Philadelphia, Pa. (38). **F E**
Scharar, Christian H., 2073 N. Main Ave., Scranton, Pa. (33). **A D E H**
SCHERMERHORN, F. AUG., 61 University Place, New York, N. Y. (36).
SCHERMERHORN, WM. C., 49 W. 23d St., New York, N. Y. (36).
Scherzer, William, 510 Home Insurance Building, Chicago, Ill. (39).
Schmid, Dr. H. Ernest, White Plains, N. Y. (25).
Schneck, Jacob, M.D., Mount Carmel, Ill. (41). **E F H**
Schobinger, John J., 2101 Indiana Ave., Chicago, Ill. (34). **B**
Schofield, Gen. J. M., Headquarters of the Army, Washington, D. C. (36).
Schöney, Dr. L., 68 East 104th St., New York, N. Y. (29). **F**
Schryver, Miss Annie A., Teachers College, New York, N. Y. (41).
Schuette, J. H., Green Bay, Wis. (34). **F E B**
Schultz, Carl H., 430–440 First Ave., New York, N. Y. (29).
Schurman, Jacob G., Pres. Cornell Univ., Ithaca, N. Y. (41). **H**
Schuyler, Philip N., Bellevue, Huron Co., Ohio (37).
Schwarz, E. A., U. S. Dep't of Agric., Washington, D. C. (29). **F**
Scott, Dr. James F., Columbia Hospital, Washington, D. C. (40). **F H**
Scott, John B., 1520 Arch St., Philadelphia, Pa. (33). **C**
Scott, Martin P., M.D., Maryland Agric. Coll., College Park, Md. (31).
Scott, W. J., M.D., 537 Prospect St., Cleveland, Ohio (37).

Scoville, S. S., M.D., Lebanon, Ohio (30). E F
Scull, Miss Sarah A., 1100 M St., Washington, D. C. (40). H
Seamon, Prof. William H., Eng., Aurora, Mo. (38).
Searing, Anna H., M.D., 52 East Ave., Rochester, N. Y. (41). G
Sebert, William F., 353 Clinton St., Brooklyn, N. Y. (41). A E
Sellhausen, Ernest A., M.D., 640 G St., N. W., Washington, D. C. (40).
Serrell, Lemuel W., 140 Nassau St., New York, N. Y. (36).
SHEAFER, A. W., Pottsville, Pa. (28).
Sheldon, Samuel, A.M., Ph.D., Polytechnic Institute, Brooklyn, N. Y. (42). B
Shepard, William A., Saratoga Springs, N. Y. (28).
Shepherd, Elizabeth, 253 W. 128th St., New York, N. Y. (39).
Sherman, Orray Taft, 379 Harvard St., Cambridge, Mass. (39).
Shonnard, Hon. Frederic, Yonkers, N. Y. (39). E F H
Shultz, Charles S., Hoboken, N. J. (31). F
Siebel, John E., Director Zymotechnic Inst., 1424 Montana St., Chicago, Ill. (39). C B F E
Siemon, Rudolph, 191 Calhoun St., Fort Wayne, Ind. (40) A F
SILVER, L. B., 172 Summit St., Cleveland, Ohio (37).
Simon, Dr. Wm., 1348 Block St., Baltimore, Md. (29). C
Slade, Elisha, Somerset, Bristol Co., Mass. (29). F
Slocum, Chas. E., M.D., Defiance, Ohio (34). F
Slosson, Prof. Edwin E., Univ. of Wyoming, Laramie, Wyo. (42).
Smedley, Sam'l L., Chief Eng., City Hall, Philadelphia, Pa. (33). D
Smillie, Thomas W., U. S. National Museum, Washington, D. C. (40). F
Smith, Prof. Albert L., Box 263, Englewood, Ill. (41). C
Smith, Andrew J., M.D., Metamora, Franklin Co., Ind. (39).
Smith, Benj. G., 11 Fayerweather St., Cambridge, Mass. (29). I
Smith, Charles H., 5484 Monroe Ave., Chicago, Ill. (33). D
Smith, De Cost, Skaneateles, N. Y. (38). H
Smith, D. T., M.D., Louisville, Ky. (39).
Smith, E. Reuel, Skaneateles, N. Y. (38).
Smith, Harlan I., East Saginaw, Mich. (41). H
Smith, Henry L., 149 Broadway, New York, N. Y. (26).
Smith, Mrs. Henry L., 149 Broadway, New York, N. Y. (26).
Smith, Prof. Herbert S. S., Coll. of New Jersey, Princeton, N. J. (29). D
Smith, Mrs. J. Lawrence, 1040 Second St., Louisville, Ky. (26).
Smith, James Hervey, Baldwin Univ., Berea, Ohio (40).
Smith, James Perrin, Ph.D., Ass't Prof. of Paleontology, Leland Stanford Junior Univ., Palo Alto, Cal. (37). C E
Smith, Miss Jane, Peabody Museum, Cambridge, Mass. (29). H
Smith, Mrs. Marshall E., 4011 Baring St., Philadelphia, Pa, (40). H I
Smith, Middleton, P. O. Box 572, Washington, D. C. (40).
Smith, Theodore W., Indianapolis, Ind. (39). C
Smith, Prof. Thomas A., Beloit, Wis. (33). B A

SMITH, USELMA C., 707 Walnut St., Philadelphia, Pa. (33). F
Smucker, Isaac, Newark, Ohio (29). H
Smyth, C. H., jr., Clinton, N. Y. (38).
Smyth, Prof. Jas. D., Burlington, Iowa (28). I
Snell, Merwin Marie, Catholic Univ. of America, Washington, D. C. (40).
Snow, B. W., Ass't Statistician, Dept. of Agric., Washington, D. C. (40).
Snyder, John F., M.D., Virginia, Cass Co., Ill. (42).
Soule, Wm., Ph.D., Alliance, Ohio (33). B C E
Southwick, E. B., Arsenal Building, Central Park, New York, N. Y. (36).
Southworth, Miss Effie A., Forestville, N. Y. (35). G
Souvielle, Mathieu, M.D., Box 355, Jacksonville, Fla. (36). B E F
Souvielle, Mrs. M., Box 355, Jacksonville, Fla. (24). A B F
Spear, Gen. Ellis, 1003 F St., N.W., Washington, D. C. (40). I
Speck, Hon. Charles, 1206 Morrison Ave., St. Louis, Mo. (27).
Spencer, Arthur Coe, B.S., Iowa Geol. Survey, Des Moines, Iowa. (41). E
SPENZER, JOHN G., M.D., 368 Central Ave., Cleveland, Ohio (37). C
Speyers, Clarence L. (36). C
Spilsbury, E. Gybbon, 13 Burling Slip, New York, N. Y. (33). E D
Spinney, L. B., Ames, Iowa (42). B
Spofford, Paul N., P. O. Box 1667, New York, N. Y. (36).
SPRAGUE, C. H., Malden, Mass. (29).
Sprague, Frank J., 182 West End Ave., New York, N. Y. (29).
Squibb, Edward Hamilton, M.D., 148 Columbia Heights, Brooklyn, N. Y. (41). F
Stam, Colin F., Chestertown, Md. (33). C F
Starek, Emil, E. M., 222 Indiana Ave., Washington, D. C. (40). C D E
Stark, Prof. Wm. B., State College, Lexington, Ky. (39). F
Steiger, George, Chem. Laboratory, U. S. Geol. Survey, Washington, D. C. (40). C E B
Steinmetz, Chas. Proteus, General Electric Co., Lynn, Mass. (40).
Stevens, Alviso B., Ann Arbor, Mich. (40). C
Stevens, Geo. T., M.D., 33 West 33d St., New York, N. Y. (28). B F
Stevens, Mrs. Sarah C., Mankato, Minn. (40).
Stevenson, Mrs. Cornelius, 237 S. 21st St., Philadelphia, Pa. (33).
Stieglitz, Dr. Julius, Univ. of Chicago, Chicago, Ill. (39). C
Stillman, Prof. John M., Palo Alto, Cal. (41).
Stine, Prof. W. M., Director Elect. Dept., Armour Institute, Chicago, Ill. (37). A C
Stockbridge, Horace E., Fargo, No. Dakota (31).
Stoek, H. H., Lehigh Univ., So. Bethlehem, Pa. (40). E
Stoller, Prof. James H., Union College, Schenectady, N. Y. (36). E F
Stone, D. D., Lansing, N. Y. (39). F
Stone, Miss Ellen Appleton, 280 Waterman St., Providence, R. I. (42). E F
Stone, Lincoln R., M.D., Newton, Mass. (31).
Stowell, John, 48 Main St., Charlestown, Mass. (21).
Stradling, Prof. George F., Hatboro, Montgomery Co., Pa. (41).

Streeruwitz, W. H. von, Austin, Texas (40).
Stubbs, W. C., Audubon Park, New Orleans, La. (40).
Studley, Prof. Duane, 410 E. Main St., Crawfordsville, Ind. (41). A
Sudworth, George B., Dept. of Agric., Forestry Div., Washington, D. C. (41)) G
Sullivan, J. A., care Dr. Sullivan, 310 Main St., Malden, Mass. (27). A
Sullivan, J. C., M.D., Cairo, Ill. (40). A
Summers, Henry E., Champaign, Ill. (42). F
Swasey, Oscar F., M.D., Beverly, Mass. (17).
Sweet, Henry N., 89 State St., Boston, Mass. (40). H D
Sweetnam, Geo. Booker, 39 St. Vincent St., Toronto, Ontario, Can. (38).

Taft, Charles E., Bay View, Station R, New York, N. Y. (37). E
Taft, Mrs. Charles E., Bay View, Station R, New York, N. Y. (37). B
TAFT, ELIHU B., Burlington, Vt. (36). H
Talbott, Mrs. Laura Osborne, 927 P St., Washington, D. C. (36).
Talbott, Dr. Thomas M., 927 P St., Washington, D. C. (40).
Talmage, Prof. James E., D.S.D., Ph.D., Curator Deseret Museum, Salt Lake City, Utah (41). C F
Taylor, F. B., Box 2019, Fort Wayne, Ind. (39).
Taylor, Hudson K., 61 Fowler St., Cleveland, Ohio. (42). C
Taylor, Prof. Jas. M., Hamilton, Madison Co., N. Y. (33). A D
Taylor, Robert S., Box 2019, Fort Wayne, Ind. (39).
Taylor, William Alton, 1516 Caroline St., N. W., Washington, D. C. (40).
Teller, George L., Fayetteville, Washington Co., Ark. (40). C
Thaw, Mrs. Mary Copley, Pittsburgh, Pa. (41). H
Theilmann, Emil, Appleton City, Mo. (41).
Thomas, Prof. M. B., Crawfordsville, Ind. (41). G
Thompson, Alton Howard, 721 Kansas Ave., Topeka, Kan. (33). H F
Thompson, Daniel G., 120 Broadway, New York, N. Y. (29).
Thompson, Mrs. Ellen P., 1729 12th St., Washington, D. C. (40). H
Thompson, Mrs. Frank, 233 South 4th St., Philadelphia, Pa. (33).
THOMPSON, FRED'K F., 283 Madison Ave., New York, N. Y. (36).
Thompson, J. L., M.D., Indianapolis, Ind. (39). F
Thruston, R. C. Ballard, Louisville, Ky. (36). E
Tiffany, Asa S., 1221 Rock Island St., Davenport, Scott Co., Iowa (27). E H
Tight, Prof. William George, Granville, Ohio (39). F
Tindall, Willoughby C., Associate Prof. of Math., Univ. of Missouri, Columbia, Mo. (40).
Todd, Albert M., Nottawa, Mich. (37). C
Townsend, Prof. Charles O., Macon, Ga. (41). F
Townsend, Clinton P., Donaldsonville, La. (40). C
Townsend, Franklin, 4 Elk St., Albany, N. Y. (4).
Townsend, Henry C., 709 Walnut St., Philadelphia, Pa. (33). I
Treat, Erastus B., Publisher and Bookseller, 5 Cooper Union, cor. 4th Ave. and 8th St., New York, N. Y. (29). F I

Trenholm, Hon. W. L., Pres. Amer. Surety Co., 160 Broadway, New York, N. Y. (35).
Trescot, T. C., 1418 L St., Washington, D. C. (40). C
Trowbridge, Luther H., 266 Woodward Ave., Detroit, Mich. (29).
Trowbridge, Mrs. M. E. D., 266 Woodward Ave., Detroit, Mich. (21).
Tryon, F. M., Room 120, Patent Office, Washington, D. C. (40).
Tudor, Prof. Joseph H., Florence, N. J. (39).
Tullock, Alonzo J., Engineer, Leavenworth, Kansas (35). D
Turner, Henry Ward, U. S. Geol. Survey, Washington, D. C. (34). E

Uline, Edwin Burton, Mishawaka, Ind. (42). G
Updegraff, Milton, Observatory, Columbia, Mo. (40). A

Vail, Prof. Hugh D., Santa Barbara, Cal. (18).
Valentine, Benj. B., Richmond, Va. (33). H
Valentine, Edw. P., Richmond, Va. (33). H
VAN BEUREN, FREDERICK T., 21 W. 14th St., New York, N. Y. (36).
Van Brunt, Cornelius, 319 E. 57th St., New York, N. Y. (28).
Van Slyke, James M., Madison, Wis. (42). F
Van Slyke, Lucius L., Agric. Exper. Station, Geneva, N. Y. (41).
Van Vleck, Frank, Pacific Railway Co., Los Angeles, Cal. (35). D
Varney, Miss May, Simon, Wayne Co., Pa. (40).
Vaux, Geo., jr., 404 Girard Building, Philadelphia, Pa. (33). E A
Veeder, Major Albert, M.D., Lyons, Wayne Co., N. Y. (36).
Vermyné, J. J. B., M.D., 2 Orchard St., New Bedford, Mass. (29). F
Villard, Fanny G., Dobbs Ferry, N. Y. (36).
Vinal, W. Irving, 1106 East Capitol St., Washington, D. C. (40). E
Volk, Ernest, Trenton, N. J. (42). H
Voorhees, Chas. H., M.D., P. O. Lock Box 120, New Brunswick, N. J. (29). F H
Vredenburgh, Edw. H., 122 So. Fitzhugh St., Rochester, N. Y. (29).

Wagenhals, Samuel, Box 382, Fort Wayne, Ind. (40). H
Wagner, Frank C., care Wm. Wagner, Ann Arbor, Mich. (34). D
Waite, M. B., Dep't of Agriculture, Washington, D. C. (37). G
Wales, Salem H., 25 E. 55th St., New York, N. Y. (36).
Walker, Byron Edmund, Toronto, Ontario, Can. (38). E
Walker, George C., 228 Michigan Ave., Chicago, Ill. (17).
Walworth, Rev. Clarence A., 41 Chapel St., Albany, N. Y. (28). E
Wanamaker, John, Postmaster General, Washington, D. C. (33).
Wappenhaus, C. F. R., U. S. Weather Bureau, Indianapolis, Ind. (39). B
Ward, Frank A., 16-26 College Ave., Rochester, N. Y. (40).
Ward, J. Langdon, 120 Broadway, New York, N. Y. (29). I
Ward, Samuel B., M.D., Albany, N. Y. (29). F C A
Wardwell, George J., Rutland, Vt. (20). D E
Ware, Wm. R., Columbia College, New York, N. Y. (36).

Warner, Hulbert H., Rochester, N. Y. (31). A
Warren, Eugene C., 611 W. Main St., Louisville, Ky. (37).
Warren, Mrs. Susan E., 67 Mt. Vernon St., Boston, Mass. (29).
Warrington, James N., 127 Park Ave., Chicago, Ill. (34). D A B
Waterhouse, A., M.D., 42 Allen St., Jamestown, N. Y. (29). F
WATERS, GEO. F., 6 Somerset St., Boston, Mass. (29). B F H E D
Watkins, John E., C. E., National Museum, Washington, D. C. (40) D
Watkins, L. D., Manchester, Mich. (34). C
Watson, Miss C. A., Salem, Mass. (31). D
Watson, Elizabeth S., Weymouth, Mass. (42). E
Watson, Thomas A., Weymouth, Mass. (42). E
Watson, Thomas L., Agric. Exper. Station, Agric. and Mechan. College, Blackburg, Va. (42).
Watters, William, M.D., 26 So. Common St., Lynn, Mass. (40).
Weaver, Gerrit E. Hambleton, A.M., 300 So. 36 St., Philadelphia, Pa. (38). F
Weed, H. E., Agricultural College, Miss. (40). F
Weed, J. N., 71 Water St., Newburgh, N. Y. (37). E I
Weeden, Hon. Joseph E., Randolph, Cattaraugus Co., N. Y. (31).
Weeks, Joseph D., Editor American Manufacturer, Pittsburg, Pa. (35). D
Wells, Mrs. C. F., 27 E. 21st St., New York, N. Y. (31). H F I D B
Wells, Samuel, 31 Pemberton Square, Boston, Mass. (24). H
Wells, William H., jr., 274 Ashland Ave., Chicago, Ill. (39). E
Werum, Jno. H., Toledo, Ohio (40).
West, Miss Nellie B., 875 Madison Ave., New York, N. Y. (38). E
Wetzler, Jos., 203 Broadway, New York, N. Y. (36).
Wheeler, Herbert A., Washington Univ., St. Louis, Mo. (33). E I
Wheeler, T. B., M.D., 123 Metcalfe St., Montreal, P. Q., Can. (11).
Wheeler, William, C.E., Concord, Mass. (41).
Whelen, Edw. S., 1520 Walnut St., Philadelphia, Pa. (33).
Whetstone, John L., Summit Ave., Mt. Auburn, Cincinnati, Ohio (30). D
White, Charles H., Med. Inspector U. S. N., care A. B. Gilman, Bradford, Mass. (34). C
White, LeRoy S., Box 324, Waterbury, Conn. (23).
White, Loomis L., 7 E. 44th St., New York, N. Y. (36).
White, Thaddeus R., 400 W. 57th St., New York, N. Y. (42). A
Whitehead, John M., Att'y at Law, Janesville, Rock Co., Wis. (41) I
Whitfield, Thomas, Ph.D., 240 Wabash Ave., Chicago, Ill. (41). C
Whiting, Mrs. Francis, Jeffersonville, Montgomery Co., Pa. (40).
Whiting, S. B., 11 Ware St., Cambridge, Mass. (33). D
Whitney, E. R., 20 North St., Binghamton, N. Y. (41).
Whitney, Joseph T. (40).
Wilbor, Rev. Wm. C., Ph.D., 498 W. Ferry St., Buffalo, N. Y. (39). F
Wilbour, Mrs. Charlotte B., Little Compton, R. I. (28).
Wilbur, Miss F. Isabel, 1719 15th St., N. W., Washington, D. C. (42). E H

Wilcox, Miss Emily T., 85 Second St., Troy, N. Y. (33). B A
Wilder, Alex., M.D., 5 No. 11th St., Newark, N. J. (29). H F I
Wilkinson, Ernest, Washington, D. C. (40).
Wilkinson, J. Henderson, 320 E. Capitol St., Washington, D. C. (35). E
Wilkinson, Mrs. L. V., Seventy Six P. O., Perry Co., Mo. (30).
Willetts, Joseph C., Skaneateles, N. Y. (29). E F H
Williams, Prof. Edward H., jr., Box 463, Bethlehem, Pa. (25). E D
Williams, H. Smith, M.D., Randall's Island, New York, N. Y. (34). F
Williams, Rev. Theodore B., 170 Meigs St., Rochester, N. Y. (41).
Williams, Prof. Thos. A., Agric. College, Brookings, So. Dak. (42). G
Willits, Edwin, Dept. of Agriculture, Washington, D. C. (40). I
Willitts, George E., 210 No. Larch St., Lansing, Mich. (39). F
WILMARTH, MRS. HENRY D., 51 Eliot St., Jamaica Plain, Mass. (40).
Wilmot, Thos. J., Commercial Cable Co., Waterville, County Kerry, Ireland (27). B
Wilson, Robert N., Macleod, Alberta, Can. (42). H
Wingate, Miss Hannah S., 2101 Fifth Ave., New York, N. Y. (31). E I
Wisser, John P., 1st Lt. 1st Artillery, U. S. A., West Point, N. Y. (33). C
Wolcott, Mrs. Henrietta L. T., Dedham, Mass. (29).
Wolff, Dr. J. E., 15 Story St., Cambridge, Mass. (36).
Woll, Fritz Wilhelm, Madison, Wis. (42). C
Wood, Alvinus B., 980 Jefferson Ave., Detroit, Mich. (34). E
WOOD, DR. ROBERT W., Jamaica Plain, Mass. (29).
WOOD, WALTER, 400 Chestnut St., Philadelphia, Pa. (33). F I
Woodland, Jesse E., Havana, N. Y. (41). F
Woodman, Dr. Durand, 80 Beaver St., New York, N. Y. (41).
Woodworth, Chauncey C., Rochester, N. Y. (41). I
Wrenshall, John C., Baltimore, Md. (40). H
Wright, Carroll D., Dept of Labor, Washington, D. C. (41). I
Wright, Rufus, 333–339 Lake St., Chicago, Ill. (37). B
Wright, S. G., La Fayette, Ind. (42). G
Würtele, Miss Minnie, Acton Vale, P. Q., Can. (32). H
Wyman, Walter, M.D., Supervising Surg. Gen., U. S. Marine Hospital Service, Washington, D. C. (40). I

Yeates, W. S., U. S. National Museum, Washington, D. C. (40).
York, Mrs. Margaret M., 937 Westminster St., Washington, D. C. (40). H
Youmans, Mrs. Celia G., Mount Vernon, N. Y. (36).
Yowell, Everett I., Station "C," Cincinnati, Ohio (41). A

Zeng, Miss Nellie E. de, Clyde, Wayne Co., N. Y. (41). B H

[1142 PATRONS, CORRESPONDING MEMBERS AND MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

## HONORARY FELLOWS.[1]

ROGERS, WILLIAM B., Boston, Mass. (1). 1881. (Born Dec. 7, 1804. Died May 30, 1882.) **B E**

CHEVREUL, MICHEL EUGÈNE, Paris, France (35). 1886. (Born Aug. 31, 1786. Died April 9, 1889.) **C**

GENTH, DR. F. A., 3937 Locust St., Philadelphia, Pa. (24). 1888. (Born May 17, 1820. Died Feb. 2, 1892.) **C E**

HALL, PROF. JAMES, Albany, N. Y. (1). 1890. **E F**

## FELLOWS.[2]

Abbe, Professor Cleveland, Meteorologist, Weather Bureau, Dept. of Agric., Washington, D. C. (16). 1874. **B A**

Abbe, Robert, 11 W. 50th St., New York, N. Y. (36). 1892.

Abert, S. Thayer, 1108 G St., N. W., Washington, D. C. (30). 1891. **A B D E I**

Alden, Prof. Geo. I., Worcester, Mass. (33). 1885. **D**

Alexander, John S., Texas Nat'l Bank, San Antonio, Texas (20). 1874. **B C D**

Allen, Joel A., American Museum of Natural History, 77th St. and 8th Ave., New York, N. Y. (18). 1875. **F**

Allen, Dr. T. F., 10 E. 36th St., New York, N. Y. (35). 1887. **G**

Alvord, Major Henry E., Lewinsville, Fairfax Co., Va. (29). 1882. **I**

Alwood, Prof. Wm. B., Agricultural and Mechanical College and Experiment Station, Blacksburg, Va. (39). 1891. **F**

Andrews, Prof. Launcelot W., Iowa City, Iowa (39). 1891. **C**

Anthony, Prof. Wm. A., Manchester, Conn. (28). 1880. **B**

Arey, Albert L., Free Academy, Rochester, N. Y. (35).

Arthur, J. C., La Fayette, Ind. (21). 1883. **G**

Ashmead, Wm. H., 1833 M St., N. W., Washington, D. C. (40). 1892. **F**

Atkinson, Edward, 31 Milk St., Boston, Mass. (29). 1881. **I D**

Atkinson, George F., Cornell Univ., Ithaca, N. Y. (39). 1892. **G**

Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29). 1882. **C**

Atwell, Charles B., 1038 Sherman St., Evanston, Ill. (36). 1890. **F**

Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29). 1886. **D A**

Avery, Elroy M., Ph.D., Woodland Hills Ave., Cleveland, Ohio (37). 1889. **B**

Ayres, Prof. Brown, Tulane Univ., New Orleans, La. (31). 1885. **B**

Babcock, S. Moulton, Madison, Wis. (33). 1885. **C**

Bailey, E. H. S., Lawrence, Douglas Co., Kan. (25). 1889. **C E**

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date following is the year when made a Fellow; the black letters at end of line are those of the sections to which the Fellow belongs.

When the name is given in small capitals, it designates that the Fellow is also a Life Member, and is entitled to the Annual Volume of Proceedings.

Bailey, Prof. Liberty H., Ithaca, N. Y. (34). 1887. G

Bailey, Prof. W. W., Brown University; residence, 6 Cushing St., Providence, R. I. (18). 1874. G

Baker, Frank, M.D., 1315 Corcoran St., Washington, D. C. (31). 1886. F H

Baker, Marcus, U. S. Geological Survey, Washington, D. C. (30). 1882. A

Baldwin, Judge Charles C., 1264 Euclid Ave., Cleveland, Ohio (37). 1891. H I

Ballard, Harlan H., 50 South St., Pittsfield, Mass. (31). 1891. E F

BARKER, PROF. G. F., 3909 Locust St., Philadelphia, Pa. (18). 1875. B C

Barnes, Prof. Chas. R., Madison, Wis. (33). 1885. G

Bartlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883. C

Bartlett, John R., Commander U. S. N., Lonsdale, R. I. (30). 1882. E B

Barus, Carl, Ph.D., 2808 N St., N. W., Washington, D. C. (33). 1887. B

Bassett, Homer F., Waterbury, Conn. (23). 1874. F

Bates, Henry H., Ph.D., U. S. Patent Office, Washington, D. C. (33). 1887. B A C D

Battle, Herbert B., Ph.D., Director N. C. Agric. Exper. Station, Raleigh, N. C. (33). 1889. C

Bauer, Louis A., U. S. C. and G. Survey, Washington, D. C. (40). 1892. A

Baur, George, Univ. of Chicago, Chicago, Ill. (36). 1889.

Bausch, Edward, P. O. Drawer 1033, Rochester, N. Y. (26). 1883. A B C F

Beal, Prof. Wm. James, Agricultural College, Ingham Co., Mich. (17). 1880. G

Beardsley, Prof. Arthur, Swarthmore College, Swarthmore, Del. Co., Pa. (33). 1885. D

Beauchamp, Rev. Wm. M., Baldwinsville, N. Y. (34). 1886. H

Becker, Dr. Geo. F., U. S. Geol. Survey, Washington, D. C. (36). 1890. E

Bell, Dr. Alex. Graham (26). 1879. B H I

Bell, Alex. Melville, 1525 35th St., Washington, D. C. (31). 1885. H

Bell, Robert, M.D., Ass't Director Geological Survey, Ottawa, Ontario, Can. (38). 1889. E F

Beman, Wooster W., 19 So. 5th St., Ann Arbor, Mich. (34). 1886. A

BENJAMIN, MARCUS, care D. Appleton & Co., 1 Bond St., New York, N. Y. (27). 1887. C

Benjamin, Rev. Raphael, M.A., 178 E. 70th St., New York, N. Y. (34). 1887. E F G H

Bessey, Prof. Charles E., Univ. of Nebraska, Lincoln, Neb. (21). 1880. G

Bethune, Rev. C. J. S., Trinity College School, Pt. Hope, Ont., Can. (18). 1875. F

Beyer, Dr. Henry G., U. S. N., U. S. Naval Acad., Annapolis, Md. (31). 1884. F

Bickmore, Prof. Albert S., American Museum of Natural History, 8th Ave. and 77th St., Central Park, New York, N. Y. (17). 1880. **H**

Bigelow, Prof. Frank H., U. S. Weather Bureau, Washington, D. C. (36). 1888. **A**

Billings, John S., Surgeon U. S. A., Surg. General's Office, Washington, D. C. (32). 1883. **F H**

BIXBY, WM. H., Cap't of Eng. U. S. A., Newport, R. I. (34). 1892. **D**

Blackham, George E., M.D., Dunkirk, N. Y. (25). 1883. **F**

Blake, Clarence J., M.D., 226 Marlborough St., Boston, Mass. (24). 1877. **B F**

Blake, Prof. Eli W., Brown Univ., Providence, R. I. (15). 1874. **B**

Blake, Francis, Auburndale, Mass. (28). 1874. **B A**

Blue, Archibald, Director of the Bureau of Mines, Toronto, Ontario, Can. (35). 1890. **I**

Boardman, Mrs. William D., 38 Kenilworth St., Roxbury, Mass. (28). 1885. **E H**

Boas, Dr. Franz, Columbian Museum, Jackson Park, Chicago, Ill. (36). 1888. **H**

Boerner, Chas. G., Vevay, Switzerland Co., Ind. (29). 1886. **A B E**

Bolley, Henry L., North Dakota Agric. Coll., Fargo, North Dakota (39). 1892. **G**

BOLTON, DR. H. CARRINGTON, University Club, New York, N. Y. (17). 1875. **C**

Bond, Geo. M., care of The Pratt & Whitney Co., Hartford, Conn. (33). 1885. **D**

Bourke, John G., Capt. 3d Cavalry, U. S. A., War Dept., Washington, D. C. (33). 1885. **H**

Bouvé, Thos. T., Boston Soc. Nat. Hist., Boston, Mass. (1). 1875. **E**

Bowditch, Prof. H. P., Jamaica Plain, Mass. (28). 1880. **F B H**

Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.

Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875. **B**

Brackett, Richard N., Associate Prof. of Chemistry, Clemson Agric. College, Fort Hill, S. C. (37). **C E**

Bradford, Royal B., Commander U. S. N., care Navy Dept, Washington, D. C. (31). 1891. **B D**

Branner, John C., Leland Stanford jr. Univ., Menlo Park, Cal. (34). 1886. **E F**

Brashear, Jno. A., Allegheny, Pa. (33). 1885. **A B D**

Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875. **E F I**

Brewster, William, 61 Sparks St., Cambridge, Mass. (29). 1884. **F**

Brinton, D. G., M.D., Media, Pa. (33). 1885. **H**

Britton, N. L., Columbia College, New York, N. Y. (29). 1882. **G E**

Broadhead, Garland Carr, University, Columbia, Mo. (27). 1879. **E**

Brooks, Wm. R., Box 714, Geneva, N. Y. (35). 1886. **A B D G**

Brown, Robert, care of Yale College Observatory, New Haven, Conn. (11). 1874.

Brown, Mrs. Robert, New Haven, Conn. (17). 1874.

Brühl, Gustav, cor. John and Hopkins Sts., Cincinnati, Ohio (28). 1886. **H**

Brush, Charles F., Brush Electric Light Co., Cleveland, Ohio (35). 1886. **B**

BRUSH, PROF. GEORGE J., Yale College, New Haven, Conn. (4). 1874. **C E**

Buckhout, W. A., State College, Centre Co., Pa. (20). 1881. **F**

Burgess, Dr. Thomas J. W., Med. Sup't, Protestant Hospital for the Insane, Montreal, P. Q., Can. (38). 1889. **G**

Burr, Prof. William H., School of Mines, 41 East 49th St., New York, N. Y. (31). 1883.

Burrill, Prof. T. J., Univ. of Illinois, Champaign, Ill. (29). 1882. **G**

Butler, A. W., Brookville, Franklin Co., Ind. (30). 1885. **F H**

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875. **C**

Calvin, Prof. Samuel, State Univ. of Iowa, Iowa City, Iowa (37). 1889. **E F**

Campbell, Prof. Douglas H., Menlo Park, Cal. (34). 1888. **G**

Canby, William M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878. **G**

Carhart, Prof. Henry S., University of Michigan, Ann Arbor, Mich. (29). 1881. **B**

Carpenter, Louis G., Agricultural College, Fort Collins, Col. (32). 1889. **A B**

Carpenter, Capt. W. L., U. S. A., Dunkirk, N. Y. (24). 1877. **F E**

Carpmael, Charles, Director of Magnetic Observatory, Toronto, Ontario, Can. (31). 1883. **B**

Carr, Lucien, Peabody Museum Archæology and Ethnology, Cambridge, Mass. (25). 1877. **H**

Casey, Thomas L., Room 79, Army Building, 39 Whitehall St., New York N. Y. (38). 1892. **F**

Chamberlain, Alexander F., Clark Univ., Worcester, Mass. (38). 1890. **H**

Chamberlin, T. C., 5041 Madison Ave., Chicago, Ill. (21). 1877. **E B F H**

Chandler, Prof. C. F., School of Mines, Columbia Coll., East 49th St., cor. 4th Ave., New York, N. Y. (19). 1875. **C**

Chandler, Prof. Charles Henry, Ripon, Wis. (28). 1883. **A B**

Chandler, Seth C., jr., 16 Craigie St., Cambridge, Mass. (29). 1882. **A**

Chanute, O., 413 E. Huron St., Chicago, Ill. (17). 1877. **D I**

Chester, Prof. Albert H., Rutgers College, New Brunswick, N. J. (29). 1882. **C F**

Chester, Prof. Fred'k D., Del. State Coll., Newark, Del. (33). 1887. **E**

Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877. **G I**

Christie, Alexander Smyth, U. S. C. and G. Survey, Washington, D. C. (39). 1891. **A B D**

Chute, Horatio N., Ann Arbor, Mich. (34). 1889. **B C A**

Clapp, Miss Cornelia M., Mt. Holyoke College, South Hadley, Mass. (31). 1883. **F**

Clark, Alvan G., Cambridgeport, Mass. (28). 1880. **A B**

Clark, Prof. John E., 445 Orange St., New Haven, Conn. (17). 1875. **A**
Clark, Wm. Bullock, Ph.D., Johns Hopkins Univ., Baltimore, Md. (37). 1891. **E**
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18). 1874. **C**
Claypole, Prof. Edw. W., 603 Buchtel Ave., Akron, Ohio (30). 1882. **E F**
Clayton, H. Helm, Readville, Mass. (34). 1887. **B**
Cloud, John W., 974 Rookery, Chicago, Ill. (28). 1886. **A B D**
Coffin, Prof. Selden J., Lafayette College, Easton, Pa. (22). 1874. **A I**
Cogswell, W. B., Syracuse, N. Y. (33). 1891. **D**
Cole, Prof. Alfred D., Denison Univ., Granville, Ohio (39). 1891. **B C**
Collett, Prof. John, Indianapolis, Ind. (17). 1874. **E**
Collin, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21). 1891. **B C**
Collingwood, Francis, Elizabeth, N. J. (36). 1888. **D**
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880. **E**
Comstock, Prof. Geo. C., Washburn Observ., Univ. of Wisconsin, Madison, Wis. (34). 1887. **A**
Comstock, J. Henry, 48 East Ave., Ithaca, N. Y. (28). 1882. **F**
Comstock, Milton L., Knox College, Galesburg, Ill. (21). 1874. **A**
Comstock, Prof. Theo. B., President Univ. of Arizona, Tucson, Arizona (24). 1877. **D E B**
Conant, Prof. L. L., Polytechnic Inst., Worcester, Mass. (39). 1892. **A**
Cook, Prof. A. J., Pomona College, Claremont, Cal. (24). 1880. **F**
Cook, Prof. Chas. Sumner, Santa Barbara, Cal. (36). 1889. **B**
Cook, Prof. Orator F., Huntington, N. Y. (40). 1892. **G**
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880. **B C**
Cooley, Prof. Mortimer E., Univ. of Michigan, Ann Arbor, Mich. (33). 1885. **D**
Cope, Prof. Edward D., 2102 Pine St., Philadelphia, Pa. (17). 1875. **F E**
Corthell, Elmer L., "The Temple," Chicago, Ill. (34). 1886. **D**
Coulter, Prof. John M., Pres. Indiana Univ., Bloomington, Ind. (32). 1884. **G**
Coulter, Prof. Stanley, La Fayette, Ind. (35). 1890. **G**
Coville, Frederick V., Dept. of Agric., Washington, D. C. (35). 1890. **G**
Cox, Prof. Edward T. (19). 1874. **E**
Cox, Hon. Jacob D., Gilman Ave., Mt. Auburn, Cincinnati, Ohio (30). 1881. **F**
Coxe, Eckley B., Drifton, Luzerne Co., Pa. (23). 1879. **D E**
Cragin, Francis W., Colorado College, Colorado Springs, Col. (29). 1890. **F E H**
Craighill, Col. Wm. P., 9 Pleasant St., Baltimore, Md. (37). 1892. **D**
Crampton, Chas. A., M.D., Office of Internal Revenue, Treasury Department, Washington, D. C. (36). 1887. **C**

Crandall, Prof. A. R., Lexington, Ky. (29). 1883. **E F**

Crawford, Prof. Morris B., Middletown, Conn. (30). 1889. **B**

Crosby, Prof. Wm. O., Boston Society of Natural History, Boston, Mass. (29). 1881. **E**

Cross, Prof. Chas. R., Mass. Institute Technology, Boston, Mass. (29). 1880. **B**

Crozier, A. A., Ann Arbor, Mich. (36). 1891. **G**

Culin, Stewart, 127 South Front St., Philadelphia, Pa. (33). 1890. **H**

Cummings, John, Cummingsville, Woburn, Mass. (18). 1890. **F**

Cushing, Henry Platt, Adelbert College, Cleveland, Ohio (33). 1888. **E**

Dall, William H., Smithsonian Institution, Washington, D. C. (18). 1874. **H F**

Dana, Edward Salisbury, New Haven, Conn. (23). 1875. **B E**

Dana, Prof. James D., New Haven, Conn. (1). 1875. **E**

Dancy, Frank B., A.B., Analytical and Consulting Chemist, Office and Laboratory, 133½ Fayetteville St., Raleigh, N. C. (33). 1890. **C**

Darton, Nelson H., U. S. Geol. Survey, Washington, D. C. (37). 1893.

Davidson, Prof. Geo., U. S. Coast and Geodetic Survey, San Francisco, Cal. (29). 1881. **A B D**

Davis, Wm. Morris, Cambridge, Mass. (33). 1885. **E B**

Dawson, Sir William, Principal McGill College, Montreal, Can. (10). 1875. **E**

Day, David F., Buffalo, N. Y. (35). 1887. **G**

Day, Fisk H., M.D., 309 Sycamore St., Lansing, Mich. (20). 1874. **E H F**

Dean, George W., P. O. Box 92, Fall River, Mass. (15). 1874. **A**

Denton, Prof. James E., Stevens Institute, Hoboken, N. J. (36). 1888. **D B A**

Derby, Orville A., San Paulo, Brazil, S. A. (39). 1890.

Dewey, Fred P., Ph.B., 621 F St. N. W., Washington, D. C. (30). 1886. **C E**

Dimmock, George, P. O. Box 15, Canobie Lake, N. H. (22). 1874. **F**

Dodge, Charles R., 1336 Vermont Ave., Washington, D. C. (22). 1874.

Dodge, Prof. James A., University of Minnesota, Minneapolis, Minn. (29). 1884. **C E**

Dodge, J. Richards, Washington, D. C. (31). 1884. **I H**

Dolbear, A. Emerson, Tufts College, Mass. (20). 1880. **B**

Doolittle, Prof. C. L., South Bethlehem, Pa. (25). 1885. **A**

Dorsey, George A., Peabody Museum, Cambridge, Mass. (39). 1892. **H**

Dorsey, Rev. J. Owen, Box 305, Takoma Park, D. C. (31). 1883. **H**

Douglass, Andrew E., 63 Pine St., New York, N. Y. (31). 1885. **H**

DRAPER, DAN'L, Ph.D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Avenue, New York, N. Y. (29). 1881. **B D F A**

Drown, Prof. Thos. M., Mass. Institute Technology, Boston, Mass. (29). 1881. **C**

Du Bois, Prof. Aug. J., New Haven, Conn. (30). 1882. **A B D**

Du Bois, Patterson, Ass't Editor S.S.T., 1031 Walnut St., Philadelphia, Pa. (33). 1887. **H C I**

Dudley, Charles B., Altoona, Pa. (23). 1882. **C B D**

Dudley, Wm. L., Prof. of Chemistry, Vanderbilt Univ., Nashville, Tenn. (28). 1881. **C**

Dudley, Prof. Wm. R., Leland Stanford jr. Univ., Palo Alto, Cal. (29). 1888. **G**

Dumble, E. T., Austin, Texas (37). 1891. **E**

Dunham, Edw. K., 53 E. 30th St., New York, N. Y. (30). 1890.

Dunnington, Prof. F. P., University Station, Charlottesville, Va. (26). 1880. **C**

Dwight, Prof. William B., Vassar College, Poughkeepsie, N. Y. (30). 1882. **E F**

Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). 1879. **A**

Eaton, Prof. D. G., 55 Pineapple St., Brooklyn, N. Y. (19). 1874. **B E**

Eaton, Prof. James R., Liberty, Mo. (29). 1885. **C B E**

Eddy, Prof. H. T., Rose Polytechnic Inst., Terre Haute, Ind. (24). 1875. **A B D**

Edison, Thos. A., Orange, N. J. (27). 1878. **B**

Egleston, Prof. Thomas, 35 W. Washington Square, New York, N. Y. (27). 1879. **C D E**

Eimbeck, William, U. S. C. and G. Survey, Washington, D. C. (17). 1874. **A B D**

Eldridge, Geo. H., care U. S. Geol. Survey, Washington, D. C. (37). 1890. **E**

Elkin, William L., Yale Coll. Observ., New Haven, Conn. (33). 1885. **A**

Ely, Theo. N., Sup't Motive Power, Penn. R. R., Altoona, Pa. (29). 1886.

Emerson, Prof. Benjamin K., Amherst, Mass. (19). 1877. **E F**

Emerson, Prof. C. F., Box 499, Hanover, N. H. (22). 1874. **B A**

Emery, Albert H., Stamford, Conn. (29). 1884. **D B**

Emery, Charles E., Bennett Building, New York, N. Y. (34). 1886. **D B A**

Emmons, S. F., U. S. Geol. Survey, Washington, D. C. (26). 1879. **E**

Engelmann, George J., M.D., 3003 Locust St., St. Louis, Mo. (25). 1875. **F H**

Ernst, Carl W., 298 Commonwealth Ave., Boston, Mass. (23). 1874. **I H**

Evermann, Prof. Barton W., U. S. Fish Commission, Washington, D. C. (39). 1891.

Eyerman, John, "Oakhurst," Easton, Pa. (33). 1889. **E C**

Fairbanks, Henry, Ph.D., St. Johnsbury, Vt. (14). 1874. **B D A**

Fairchild, David G., Dept. of Agric., Washington, D. C. (40). 1892. **G**

Fairchild, Prof. H. L., University of Rochester, Rochester, N. Y. (28). 1883. **E F**

Fanning, John T., Consulting Eng., Kasota Block, Minneapolis, Minn. (29). 1885. **D**

Fargis, Rev. Geo. A., Georgetown College, Georgetown, D. C. (40). 1892.

Farlow, Dr. W. G., 29 Holyoke House, Cambridge, Mass. (20). 1875. **G**

Farquhar, Henry, Coast Survey Office, Washington, D. C. (33). 1886. **A I F B**

Fernald, Prof. M. C., State Agric. College, Orono, Me. (22). 1883. **B A**

Fernow, Bernhard E., Chief of Forestry Division, Dep't of Agriculture, Washington, D. C. (31). 1887. **G I**

Firmstone, F., Easton, Pa. (33). 1887. **D**

Fiske, Thos. S., A.M., Ph.D., Columbia College, New York, N. Y. (38). 1891. **A**

Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874. **I E**

Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883. **H**

Fletcher, James, Dominion Entomologist, Experimental Farm, Ottawa, Ontario, Can. (31). 1883. **F**

Fletcher, Dr. Robert, Army Medical Museum, Washington, D. C. (29). 1881. **F H**

Flint, Albert S., Washburn Observ., Madison, Wis. (30). 1887. **A**

Flint, James M., Surgeon U. S. N., Smithsonian Institution, Washington, D. C. (28). 1882. **F**

Foote, Dr. A. E., 4116 Elm Ave., Philadelphia, Pa. (21). 1874. **E C**

Forbes, Prof. S. A., Univ. of Illinois, Champaign, Ill. (27). 1879. **F**

Fox, Oscar C., U. S. Patent Office, Washington, D. C. (36). 1891. **B D A**

Foye, Prof. J. C., Lawrence Univ., Appleton, Wis. (29). 1884. **C B**

Franklin, William S., Ames, Iowa (36). 1892.

FRAZER, DR. PERSIFOR, Drexel Building, Room 1042, Philadelphia, Pa. (24). 1879. **E C**

Frazier, Prof. B. W., The Lehigh University, So. Bethlehem, Pa. (24). 1882. **E C**

Frear, Wm., State College, Centre Co., Pa. (33). 1886. **C**

Freer, Prof. Paul C., Ann Arbor, Mich. (39). 1891. **C**

French, Prof. Thomas, jr., Ridgeway Ave., Avondale, Cincinnati, Ohio (30). 1883. **B**

Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880. **A**

Frost, Edwin Brant, Hanover, N. H. (38). 1890. **A B**

Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882. **F**

Fuller, Prof. Homer T., Polytechnic Inst., Worcester, Mass. (35). 1891. **C E**

Fulton, Robert B., Chancellor Univ. of Miss., Prof. of Physics and Astronomy, University, Miss. (21). 1887. **B A**

Gaffield, Thomas, 54 Allen St., Boston, Mass. (29). 1889. **C B**

Gage, Simon Henry, Ithaca, N. Y. (28). 1881. **F**

Galbraith, Prof. John, Toronto, Ontario, Can. (38). 1889.

Galloway, B. T., Dep't of Agriculture, Washington, D. C. (37). 1890. **G**

Gannett, Henry, U. S. Geological Survey, Washington, D. C. (33). 1884. **E I A**

Gardiner, Dr. Edward G., Massachusetts Institute Technology, Boston, Mass. (29). 1890. **F**

Garland, Rev. Dr. L. C., Chancellor Vanderbilt University, Nashville, Tenn. (25). 1877. **B**

Gatschet, Dr. Albert S., Box 333, Washington, D. C. (30). 1882. **H**

Gibbs, Prof. J. Willard, New Haven, Conn. (33). 1885. **B**

Gilbert, G. K., U. S. Geological Survey, Washington, D. C. (18). 1874. **E**

Gill, Prof. Theo., Smithsonian Inst., Washington, D. C. (17). 1874. **F**

Gillette, Clarence P., Fort Collins, Col. (37). 1893.

Gilman, Daniel C., President Johns Hopkins University, Baltimore, Md. (10). 1875. **E H**

Glenn, William, 1348 Block St., Baltimore, Md. (33). 1893. **C**

Goessman, Prof. C. A., Mass. Agricultural College, Amherst, Mass. (18). 1875. **C**

Goff, E. S., 1113 University Ave., Madison, Wis. (35). 1889.

Gold, Theodore S., West Cornwall, Conn. (4). 1887. **B C**

Goldschmidt, S. A., Ph.D., 43 Sedgwick St., Brooklyn, N. Y. (24). 1880. **C E B**

Goldsmith, Edw., 658 No. 10th St., Philadelphia, Pa. (29). 1892. **C B**

Gooch, Frank A., Yale College, New Haven, Conn. (25). 1880. **C**

Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875. **G**

Goode, G. Brown, Curator Nat'l Museum, Washington, D. C. (22). 1874.

Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1879. **A H**

Gould, Dr. B. A., Cambridge, Mass. (2). 1875. **A B**

GRANT, MRS. MARY J., Brookfield, Conn. (23). 1874. **A**

Grant, Ulysses S., Geol. Survey of Minnesota, Minneapolis, Minn. (39). 1893. **F**

Gratacap, L. P., Ph.B., 77th St. and 8th Ave., New York, N. Y. (27). 1884. **C E F**

Gray, Elisha, Sc.D., Highland Park, Ill. (32). 1883. **B**

Gray, Prof. Thomas, Terre Haute, Ind. (38). 1889.

Green, Arthur L., La Fayette, Ind. (33). 1888. **C**

Green, Traill, M.D., Easton, Pa. (1). 1874. **C F**

Griffith, Ezra H., 5656 Washington Ave., Chicago, Ill. (39). 1892. **B F**

Grimes, J. Stanley, Room 18, 115 Monroe St., care Newark Life Ins. Co., Chicago, Ill. (17). 1874. **E H**

Grinnell, George Bird, 40 Park Row, New York, N. Y. (25). 1885. **F E**

Griswold, Leon Stacy, 238 Boston St., Dorchester, Mass. (38). 1893. **E**

Gulley, Prof. Frank A., College Station, Texas (30). 1883.

Hague, Arnold, U. S. Geol. Survey, Washington, D. C. (26). 1879.

Haines, Reuben, Haines and Chew St., Germantown, Philadelphia, Pa. (27). 1889. **C B**

Hale, Albert C., Ph.D., No. 551 Putnam Ave., Brooklyn, N. Y. (29). 1886. **C B**

Hale, Geo. E., Director of the Observatory, Univ. of Chicago, Chicago Ill. (37). 1891. **A B C**

Hale, Horatio, Clinton, Ontario, Can. (30). 1882. **H**

Hall, Prof. Asaph, 2715 N St., Georgetown, D. C. (25). 1877. **A**

Hall, Asaph, jr., Univ. of Mich, Ann Arbor, Mich. (38). 1890. **A**

Hall, Prof. C. W., 803 Univ. Ave. So., Minneapolis, Minn. (28). 1883. **E**

Hall, Prof. Edwin H., 5 Avon St., Cambridge, Mass. (29). 1881. **B**

Hall, Prof. Lyman B., Haverford College, Haverford, Pa. (31). 1884. **C**

Hallock, Dr. William, Columbia College, New York, N. Y. (40). 1893. **B E**

Hallowell, Miss Susan M., Wellesley Coll., Wellesley, Mass. (33). 1890. **G**

Halsted, Byron D., New Jersey Agricultural Experiment Station, New Brunswick, N. J. (29). 1883. **G**

Hambach, Dr. G., 1319 Lami St., St. Louis, Mo. (26). 1891. **F E**

HANAMAN, C. E., Troy, N. Y. (19). 1883. **F**

Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883. **A**

Hargitt, Prof. Charles W., Syracuse, N. Y. (38). 1891. **F**

HARKNESS, PROF. WILLIAM, U. S. N. Observatory, Washington, D. C. (26). 1878. **A B C D**

Harris, Uriah R., Lieutenant U. S. N., U. S. Naval Acad., Annapolis, Md. (34). 1886. **A**

Harris, W. T., Lock Box 1, Concord, Mass. (27). 1887. **H I**

Hart, Edw., Ph.D., Easton, Pa. (33). 1885. **C**

Hasbrouck, Prof. I. E., 364 Carlton Ave., Brooklyn, N. Y. (23). 1874. **D A I**

HASTINGS, C. S., Sheffield Scientific School of Yale College, New Haven, Conn. (25). 1878. **B**

Haupt, Prof. Lewis M., University of Pennsylvania, Philadelphia, Pa. (32). 1885. **I D E**

Hay, Prof. O. P., Irvington, Ind. (37). 1889. **E F**

Haynes, Henry W., 239 Beacon St., Boston, Mass. (28). 1884. **H**

Heal, Wm. E., Marion, Ind. (39). 1891. **A**

Heitzmann, Dr. Charles, 39 W. 45th St., New York, N. Y. (36). 1890.

Henshaw, Henry W., Bureau of Ethnology, Washington, D. C. (24). 1877. **H**

Hering, Rudolph, Civil and Sanitary Engineer, 277 Pearl St., New York, N. Y. (33). 1885. **D E I**

Hervey, Rev. A. B., President St. Lawrence University, Canton, N.Y. (22). 1879. **F**

Hilgard, Prof. E. W., University of California, Berkeley, Cal. (11). 1874. **C E B**

Hill, Robert Thomas, U. S. Geol. Survey, Washington, D. C. (36). 1889. **E**

Himes, Prof. Charles F., Carlisle, Pa. (29). 1882. **B C**

Hinrichs, Dr. Gustavus, 3132 Lafayette Ave., St. Louis, Mo. (17). 1874. **C B**

Hirschfelder, Chas. A., Vice Consul U. S. A., Toronto, Ontario, Can. (33). 1887. **H**

Hitchcock, Albert Spear, Manhattan, Kan. (39). 1892. **G**

HITCHCOCK, PROF. CHARLES H., Hanover, N. H. (11). 1874. **E**

Hobbs, William Herbert, Ph.D., Madison, Wis. (41). 1893. **E**

Hoffmann, Dr. Fred., "Rundschau," P. O. Box 1680, New York, N. Y. (28). 1881. **C F**

Hoffmann, Dr. Walter J., Bureau of Ethnology, Washington, D. C. (38). 1890. **H**

Holden, Prof. E. S., Mt. Hamilton, Cal. (23). 1875. **A**

Hollick, Arthur, Columbia College, New York, N. Y. (31). 1892. **G E**

Holm, Theodor, U. S., Natl. Museum, Washington, D. C. (40). 1892. **F**

Holman, Silas W., Massachusetts Institute of Technology, Boston, Mass. (31). 1883. **B**

Holmes, Prof. Jos. A., Chapel Hill, N. C. (33). 1887. **E F**

Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881. **H**

Holmes, Wm. H., Bureau of Ethnology, Smithsonian Institution, Washington, D. C. (30). 1883. **H**

Holway, E. W. D., Decorah, Iowa (33). 1890. **G**

Horton, S. Dana, Pomeroy, Ohio (37). 1889. **I**

Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30). 1883. **B H**

Hotchkiss, Major Jed., Staunton, Va. (31). 1883. **E H I**

Hough, Prof. G. W., Northwestern Univ., Evanston, Ill. (15). 1874. **A**

Hough, Walter, U. S. National Museum, Washington, D. C. (38). 1890.

Hovey, Rev. Horace C., 60 High St., Newburyport, Mass. (29). 1883. **E H**

Howard, Prof. Curtis C., 97 Jefferson Ave., Columbus, Ohio (38). 1892. **C**

Howard, Leland O., Dep't of Agric., Washington, D. C. (37). 1889. **F**

Howe, Charles S., Prof. of Mathematics, Case School of Applied Science, Cleveland, Ohio (34). 1891. **A**

Howe, Jas. Lewis, 539 4th St., Louisville, Ky. (36). 1888. **C**

Howell, Edwin E., 612 17th St., N. W., Washington, D. C. (25). 1891. **E**

Hulst, Rev. Geo. D., 15 Himrod St., Brooklyn, N. Y. (29). 1887. **F**

Hunt. Alfred E., 116 Water St., Pittsburgh, Pa. (35). 1891. **C D**

Hunt, George, 119 Prospect St., Providence, R. I. (9). 1874.

Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875. **E**

Hyde, Prof. E. W., Station D, Cincinnati, Ohio (25). 1881. **A**

Iddings, Joseph P., U. S. Geological Survey, Washington, D. C. (31). 1884. **E**

Irby, Prof. B., Raleigh, N. C. (37). 1891. **F**

Jack, John G., Jamaica Plain, Mass. (31). 1890. **G**

Jackson, Robert T., 33 Gloucester St., Boston, Mass. (37). 1890. **F**

Jacobus, David S., Stevens Institute, Hoboken, N. J. (36). 1889. **D B A**

Jacoby, Harold, Columbia College, New York, N. Y. (38). 1891. **A**

Jacoby, Henry S., in charge of Bridge Engineering and Graphics, College of Civil Eng., Cornell Univ., Ithaca, N. Y. (36). 1892. **D**

James, Edmund J., Ph.D., Univ. of Penn., Philadelphia, Pa. (33). 1887. **I**

James, Jos. F., M.S., U. S. Dept. of Agric., Washington, D. C. (30). 1882. **F E**

Jastrow, Dr. Jos., Univ. of Wisconsin, Madison, Wis. (35). 1887. **H F**

Jayne, Horace F., 1826 Chestnut St., Philadelphia, Pa. (29). 1884. **F H**

Jeffries, B. Joy, M.D., 15 Chestnut St., Boston, Mass. (29). 1881. **F H**

Jenkins, Edw. H., New Haven, Conn. (33). 1885. **C**

Jenkins, Prof. Oliver P., Leland Stanford jr. Univ., Menlo Park, Cal. (39). 1891. **F**

Jenks, Elisha T., Middleborough, Mass. (22). 1874. **D**

Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874. **B**

Jesup, Prof. Henry G., Dartmouth College, Hanover, N. H. (36). 1891. **F**

Jesup, Morris K., 44 Pine St., New York, N. Y. (29). 1891. **I**

Jewell, Theo. F., Commander U. S. Navy, Navy Yard, Washington, D. C. (25). 1882. **B**

Jillson, Dr. B. C., 6224 Station St., E.E., Pittsburgh, Pa. (14). 1881. **E H F**

Johnson, Arnold Burges, Chief Clerk Light House Board, Washington, D. C. (35). 1888. **B F I**

Johnson, John B., Washington Univ., St. Louis, Mo. (33). 1886. **D**

Johnson, Lawrence C., U. S. Geol. Survey, Gainesville, Fla. (33). 1887.

Johnson, Otis C., 52 Thayer St., Ann Arbor, Mich. (34). 1886. **C**

Jones, Prof. Marcus E., Salt Lake City, Utah. (40). 1893.

Jordan, Prof. David S., Palo Alto, Menlo Park P. O., Cal. (31). 1883. **F**

Julien, A. A., New York Acad. of Sciences, New York, N. Y. (24). 1875. **E C**

Kedzie, Mrs. Nellie S., Manhattan, Kan. (34). 1890. **I F**

Kedzie, Prof. Robert C., Agricultural College, Mich. (29). 1881. **C**

Kellerman, Prof. William A., Ohio Univ., Columbus, Ohio. (41). 1893. **G**

Kellicott, David S., Columbus, Ohio (31). 1883. **F**

Kemp, James F., School of Mines, Columbia College, New York, N. Y. (36). 1888. **E**

Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882. **A**

Kent, William, Passaic, N. J. (26). 1881. **D I**

Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1883. **A B**

Keyes, Charles R., 926 Ninth St., Des Moines, Iowa (37). 1890.

Kinealy, John H., Washington Univ., St. Louis, Mo. (36). 1891. **D**

King, F. H., Experiment Station, Madison, Wis. (32). 1892. **E F**

Kinnicutt, Dr. Leonard P., Polytechnic Inst., Worcester, Mass. (28). 1883. **C**

Kirkwood, Prof. Daniel, Arlington Ave., Riverside, Cal. (7). 1874. **A**

Klotz, Otto Julius, 437 Albert St., Ottawa, Ontario, Can. (38). 1889.

Knorr, Aug. E., 1109 14th St., N. W., Washington, D. C. (40). 1893. **C**

Knowlton, Frank H., U. S. National Museum, Washington, D. C. (33). 1893. **G E**

Kunz, G. F., care Messrs. Tiffany & Co., Union Square, New York, N. Y (29). 1883. **E H C**

Lacoe, Ralph D., Pittston, Pa. (31). 1893. **E F**

Ladd, Prof. E. F., Agric. College, Fargo, No. Dakota (36). 1889. **C**

Laflamme, Prof. J. C. K., Laval Univ., Quebec, Can. (29). 1887. **E B**

LaFlesche, Francis, Indian Bureau, Interior Dep't, Washington, D. C. (33). 1885. **H**

Landreth, Prof. Olin H., Vanderbilt Univ., Nashville, Tenn. (28). 1883. **D**

Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 1882. **F H**

Langley, Prof. J. W., 136 First Ave., Pittsburgh, Pa. (23). 1875. **C B**

Langley, Prof. S. P., Secretary Smithsonian Institution, Washington, D. C. (18). 1874. **A B**

Lanza, Prof. Gaetano, Mass. Institute of Technology, Boston, Mass. (29). 1882. **D A B**

Larkin, Edgar L., Director Knox College Observatory, Galesburg, Ill. (28). 1883. **A**

Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15). 1874. **C**

Laudy, Louis H., Ph.D., School of Mines, Columbia College, New York, N. Y. (28). 1890. **C**

Lawrence, George N., 45 E. 21st St., New York, N. Y. (7). 1877. **F**

Lazenby, Prof. Wm. R., Columbus, Ohio (30). 1882. **B I**

Leavenworth, Francis P., Haverford College P. O., Montgomery Co., Pa. (30). 1888. **A**

LeBrun, Mrs. Michel, 245 West 23d St., New York, N. Y. (35). 1892. **F**

LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881. **E F**

Ledoux, Albert R., Ph.D., 9 Cliff St., New York, N. Y. (26). 1881. **C**

Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874. **C F**

Lehmann, G. W., Ph.D., 412 East Lombard St., Baltimore, Md. (30). 1885. **C B**

Lesley, Prof. J. Peter, State Geologist of Pennsylvania, 1008 Clinton St., Philadelphia, Pa. (2). 1874. **E**

Leverett, Frank, Denmark, Iowa (37). 1891. **E**

Libbey, Prof. William, jr., Princeton, N. J. (29). 1887. **E F**

Lindahl, Josua, Ph.D., State Geologist, Springfield, Ill. (40). 1892. **F E**

Lindenthal, Gustav, C.E., 45 Cedar St., New York, N. Y. (37). 1891. **I**

Lintner, J. A., N. Y. State Entomologist, Room 27, Capitol, Albany, N. Y. (22). 1874. **F**

Lloyd, John Uri, Pharmaceutical Chemist, Court and Plum Sts., Cincinnati, Ohio (38). 1890. **C F**

Lloyd, Mrs. Rachel, Box 675, Lincoln, Neb. (31). 1889. **C**

Locy, Prof. Wm. A., Lake Forest, Ill. (34). 1890. **F**

Loeb, Morris, Ph.D., 37 E. 38th St., New York, N. Y. (36). 1889. **C**

Lord, Prof. Nat. W., State Univ., Columbus, Ohio (29). 1881. **C**

Loud, Prof. Frank H., 1203 N. Tejon St., Colorado Springs, Col. (29). **A B**

Loughridge, Dr. R. H., Ass't Prof. Agric. Chem. and Agric. Geol., Univ. of California, Berkeley, Cal. (21) 1874. **E C**

Love, Edward G., 69 E. 54th St., New York, N. Y. (24). 1882. **C**

Low, Seth, Pres. Columbia Coll., New York, N. Y. (29). 1890.

Lyle, David Alexander, Captain Ordnance Dept. U. S. A., Ordnance Office, War Dept., Washington, D. C. (28). 1880. **D**

Lupton, Prof. N. T., Auburn, Lee Co., Ala. (17). 1874. **C**

Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

McAdie, Alexander George, U. S. Weather Bureau, Washington, D. C. (40). 1892. **B**

McBride, Prof. Thomas H., Iowa City, Iowa (38). 1890. **G**

McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., Portland, Oregon. (29). 1881.

McCreath, Andrew S., 223 Market St., Harrisburg, Pa. (33). 1889. **C E**

McDonnell, Prof. Henry B., College Park, Md. (40). 1893. **C**

McGee, Dr. Anita Newcomb, 2026 Hillyer Place, Washington, D. C. (37). 1892. **H**

McGee, W. J., U. S. Geol. Survey, Washington, D. C. (27). 1882. **H E**

McGill, John T., Ph.D., Vanderbilt Univ., Nashville, Tenn. (36). 1888. **C**

McGregory, Prof. J. F., Colgate Univ., Hamilton, N. Y. (35). 1892.

McGuire, Joseph D., Ellicott City, Md. (30). 1891. **H**

McMahon, James, Ithaca, N. Y. (36). 1891. **A**

McMurtrie, William, 106 Wall St., New York, N. Y. (22). 1874. **C**

McNeill, Malcolm, Lake Forest, Ill. (32). 1885. **A**

McRae, Austin Lee, Rolla, Mo. (39). 1891. **B**

Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881. **C**

Macfarlane, Prof. A., Univ. of Texas, Austin, Texas (34). 1886. **B A**

Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882. **F**

Magie, Prof. William F., College of New Jersey, Princeton, N. J. (35). 1887.

Mallery, Col. Garrick, U. S. Army, Bureau of Ethnology, Washington, D. C. (26). 1879. **H**

MANN, B. PICKMAN, 1918 Sunderland Place, Washington, D. C. (22). 1874. **I F**

Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874. **E**

Mark, Prof. E. H., Louisville, Ky. (39). 1893. **B**

Marsh, Prof. C. Dwight, Ripon, Wis. (34). 1893. **F E**

MARSH, PROF. O. C., Yale College, New Haven, Conn. (15). 1874. **F H**

Martin, Artemas, U. S. Coast Survey, Washington, D. C. (38). 1890. **A**

Martin, Prof. Daniel S., 236 West 4th St., New York, N. Y. (23). 1879. **E F**

Martin, Prof. H. Newell, Johns Hopkins University, Baltimore, Md. (27). 1880. **F H**

Martin, Miss Lillie J., Girl's High School, San Francisco, Cal. (32). 1886. **F C**

Martin, Prof. Wm. J., Davidson College, N. C. (31). 1884. **C E**

Marvin, C. F., Signal Office, Washington, D. C. (39). 1892. **B**

Mason, Prof. Otis T., Nat'l Museum, Washington, D. C. (25). 1877. **H**

Mason, Dr. William P., Prof. Rensselaer Polytechnic Inst., Troy, N. Y. (31). 1886. **C**

Matthews, Dr. Washington, 1262 New Hampshire Ave., cor. 21st St., N. W., Washington, D. C. (37). 1888. **H**

Maxwell, Walter, Dept. of Agriculture, Washington, D. C. (40). 1892. **C**

Mayer, Prof. A. M., Stevens Inst. of Technology, Hoboken, N. J. (19). 1874.

Meehan, Thomas, Germantown, Pa. (17). 1875. **G**

Mees, Prof. Carl Leo, Rose Polytechnic Inst., Terre Haute, Ind. (24). 1876. **B C**

Mendenhall, Prof. T. C., U. S. C. and G. Survey, Washington, D. C. (20). 1874. **B**

Menocal, Aniclto G., C.E., U. S. N., Navy Yard, Washington, D. C. (36). 1888. **D**

Mercer, H. C., Doylestown, Bucks Co., Pa. (41). 1893.

Merrill, Frederick J. H., Ph.B., Ass't Director New York State Museum, Albany, N. Y. (35). 1887. **E**

Merriman, C. C., 1910 Surf St., Lake View, Chicago, Ill. (29). 1880. **F**

Merriman, Prof. Mansfield, So. Bethlehem, Pa. (32). 1885. **A D**

Merritt, Ernest, Ithaca, N. Y. (33). 1890.

Metz, Charles L., M.D., Madisonville, Hamilton Co., Ohio (30). 1885. **H**

Michael, Mrs. Helen Abbott, Torwood, Bonchurch, Isle of Wight, England (33). 1885. **C F**

Michelson, A. A., Master U. S. N., 7 Rockwell St., Cleveland, Ohio (26). 1879. **B**

Miles, Prof. Manly, Lansing, Mich. (29). 1890. **F**

Mills, Wesley, Montreal, P. Q., Can. (31). 1886. **F H**

Millspaugh, C. F., M.D. (40). 1892. **G**

Minot, Dr. Charles Sedgwick, Harvard Medical School, Back Bay, Boston, Mass. (28). 1880. **F**

Minot, Francis, M.D., 65 Marlborough St., Boston, Mass. (29). 1884.

Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882. **C**

Moler, Geo. S., 106 University Ave., Ithaca, N. Y. (38). 1892.

Moody, Robert O., Fair Haven Heights, New Haven, Conn. (35). 1892. **F**

Mooney, James, Bureau of Ethnology, Washington, D. C. (38). 1890. **H**

Moore, Eliakim Hastings, 5311 Washington Ave., Chicago, Ill. (39). 1891. **A**

Moore, Prof. J. W., M.D., Lafayette College, Easton, Pa. (22). 1874. **B D A**

Moore, Veranus A., M.D., Bureau of Animal Industry, Dept. of Agric., Washington, D. C. (40). 1892. **F**

Moorehead, Warren K., Xenia, Ohio (38). 1890. **H**

Morley, Prof. Edward W., 23 Cutler St., Cleveland, Ohio (18). 1876. **C B E**

Morong, Rev. Thomas, Columbia College, New York, N. Y. (35). 1887. **G**

Morse, Prof. E. S., Salem, Mass. (18). 1874. **F H**

Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875. **B C**

Moser, Lieut. Comd'r Jeff. F., U. S. N., Hydrographic Inspector, C. and G. Survey, Washington, D. C. (28). 1889. **E**

Moses, Prof. Thomas F., Urbana University, Urbana, Ohio (25). 1883. **H F**

Munroe, Prof. C. E., Columbian Univ., Washington. D. C. (22). 1874. **C**

Murdoch, John, Rock, Plymouth Co., Mass. (29). 1886. **F H**

Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881. **F**

Myers, John A., Agric. Exper. Station, Morgantown, W. Va. (30). 1889. **C**

Nagle, Prof. James C., A. and M. Coll., College Station, Texas. (40). 1893. **D B**

Nason, Frank L., 5 Union St., New Brunswick, N. J. (36). 1888. **E**

Nason, Prof. H. B., Rensselaer Polytechnic Institute, Troy, N. Y. (13). 1874. **C E**

Nef, J. U., Clark Univ., Worcester, Mass. (39). 1891. **C**

Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882. **A B D**

Newberry, Prof. Spencer Baird, Ithaca, N. Y. (33). 1887. **C**

Newcomb, Prof. S., Navy Dep't, Washington, D. C. (13). 1874. **A B**

Newton, Hubert A., New Haven, Conn. (6). 1874. **A**

Nichols, Ernest Fox, Hamilton, N. Y. (41). 1893. **B**

Nichols, E. L., Ph.D., Cornell Univ., Ithaca, N. Y. (28). 1881. **B C**

Nicholson, Prof. H. H., Box 675, Lincoln, Neb. (36). 1888.

Niles, Prof. W. H., Cambridge, Mass. (16). 1874.

Nipher, Prof. F. E., Washington Univ., St. Louis, Mo. (24). 1876. **B**

Nolan, Edw. J., M.D., Academy of Natural Sciences, Philadelphia, Pa. (29). 1890. **F**

NORTON, PROF. THOMAS H., Univ. of Cincinnati, Cincinnati, Ohio (35). 1887. **C**

Novy, Dr. Frederick G., University of Mich., Ann Arbor, Mich. (36). 1889. **C**

Noyes, Prof. Wm. A., Rose Polytechnic Inst., Terre Haute, Ind. (32). 1885. **C**

Nuttall, Mrs. Zelia, care Peabody Museum, Cambridge, Mass. (35). 1887. **H**

Nutting, Prof. Charles C., State Univ. of Iowa, Iowa City, Iowa (40). 1892. **F**

Ogden, Herbert G., U. S. C. and G. Survey, Washington, D. C. (38). 1891. **E**

Oliver, Charles A., M.D., 1507 Locust St., Philadelphia, Pa. (33). 1886. **F H B**

Oliver, Prof. James E., 7 Central Ave., Ithaca, N. Y. (7). 1875. **A B I**

Ordway, Prof. John M., Tulane University, New Orleans, La. (9). 1875. **C**

Orndorff, Dr. William Ridgely, Cornell Univ., Ithaca, N. Y. (41). 1893. **C**

Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875. **E**

Osborn, Henry F., Columbia College, New York, N. Y. (29). 1883.

Osborn, Herbert, Ames, Iowa (32). 1884. **F**

Osmond, Prof. I. Thornton, State College, Centre Co., Pa. (33). 1889. **B A C**

Packard, Dr. A. S., 115 Angell St., Providence, R. I. (16). 1875. **F E**

Paine, Cyrus F., 305 Ellwanger & Barry Building, Rochester, N. Y. (12). 1874. **B A**

Paine, Nathaniel, Worcester, Mass. (18). 1874. **H**

Palfray, Hon. Charles W., Salem, Mass. (21). 1874.

Pammel, Prof. L. H., Iowa Agricultural College, Ames, Iowa (39). 1892.

Parke, John G., Gen. U. S. A., 16 Lafayette Square, Washington, D. C. (29). 1881. **D**

PARKHURST, HENRY M., 173 Gates Ave., Brooklyn, N. Y. (23). 1874. **A**

Patrick, Geo. E., Ames, Iowa (36). 1890. **C**

Patterson, Harry J., College Park, Prince George's Co., Md. (36). 1890. **C**

Paul, Prof. Henry M., U. S. Naval Observatory, Washington, D. C. (33). 1885. **A B**

Peabody, Selim H., 608 Rand McNally Building, Chicago, Ill. (17). 1885. **D B F**

Peary, Robert E., C.E., U. S. N., United States Navy Yard, League Island, Philadelphia, Pa. (36). 1892. **D**

Pedrick, Wm. R., Lawrence, Mass. (22). 1875.

Peet, Rev. Stephen D., Avon, Ill. (24). 1881. **H**

Penrose, Dr. R. A. F., 1331 Spruce St., Philadelphia, Pa. (38). 1890. **E**

Perkins, Prof. George H., Burlington, Vt. (17). 1882. **H F E**

Peter, Alfred M., 171 Rose St., Lexington, Ky. (29). 1890. **C**

Peter, Dr. Robert, Kentucky Geol. Survey, Lexington, Ky. (29). 1881. **C**

Peters, Edw. T., P. O. Box 265, Washington, D. C. (33). 1889. **I**

Pettee, Prof. William H., 52 Thompson St., Ann Arbor, Mich. (24). 1875. **E**

Phillips, Prof. A. W., New Haven, Conn. (24). 1879.
Phillips, Prof. Francis C., 59 Sherman Ave., Allegheny, Pa. (36). 1889. **C**
Phillips, Henry, jr., 1811 Walnut St., Philadelphia, Pa. (32). 1887. **H I**
Phippen, Geo. D., Salem, Mass. (18). 1874. **F**
Pickering, Prof. E. C., Director of Observatory, Cambridge, Mass. (18). 1875. **A B**
Pilling, James C., Box 591, Washington, D. C. (28). 1882. **F H I**
Pillsbury, Prof. John H., Smith College, Northampton, Mass. (23). 1885. **F H**
Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 1319 Walnut St., Philadelphia, Pa. (27). 1882. **E**
Plumb, Charles S., Purdue Univ., La Fayette, Ind. (36). 1890.
Pohlman, Dr. Julius, Buffalo, N. Y. (32). 1884. **E F**
Porter, Thos. C., LL.D., Lafayette College, Easton, Pa. (33). 1887. **G**
Potter, William B., Washington Univ., St. Louis, Mo. (25). 1879.
Powell, Major J. W., U. S. Geologist, 910 M St., N. W., Washington, D. C. (23). 1875. **E H**
Power, Prof. Frederick B., 225 Gregory Ave., Passaic, N. J. (31). 1887. **C**
Prentiss, Prof. A. N., Cornell Univ., Ithaca, N. Y. (35). 1887. **G**
Prentiss, D. Webster, M.D., 1101 14th St., N. W., Washington, D. C. (29). 1882. **F**
Prentiss, Robert W., Prof. of Mathematics and Astronomy, Rutgers College, New Brunswick, N. J. (40). 1891. **A**
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875. **C**
Preston, E. D., Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (37). 1889. **A E**
Prosser, Charles S., Prof. of Geology, Washburn College, Topeka, Kan. (33). 1891. **E F**
Pulsifer, Wm. H., Newton Centre, Mass. (26). 1879. **A H**
Pumpelly, Prof. Raphael, U. S. Geological Survey, Newport, R. I. (17). 1875. **E I**
Putnam, Prof. F. W., Curator Peabody Museum American Archæology and Ethnology, Cambridge, Mass. (Address as Permanent Secretary A. A. A. S., Salem, Mass.) (10). 1874. **H**

Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rathbun, Richard, U. S. National Museum, Washington, D. C. (40). 1892. **F**
Rau, Eugene A., Bethlehem, Pa. (33). 1890. **G**
Raymond, Rossiter W., 13 Burling Slip, New York, N. Y. (15). 1875. **E I**
Redfield, J. H., 216 W. Logan Square, Philadelphia, Pa. (1). 1874. **G**
Rees, Prof. John K., Columbia College, New York, N. Y. (26). 1878. **A E B**
Reese, Charles L., The Citadel, Charleston, S. C. (39). 1892. **C**
Reese, Jacob, 400 Chestnut St., Philadelphia, Pa. (33). 1891. **D B**

Reid, Harry Fielding, Case School of Applied Science, Cleveland, Ohio. (36). 1893. **B**

Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875. **C**

Rice, Prof. Wm. North, Wesleyan University, Middletown, Conn. (18). 1874. **E F**

Richards, Prof. Charles B., 137 Edwards St., New Haven, Conn. (33). 1885. **D**

Richards, Edgar, 1621 H St., Washington, D. C. (31). 1886. **C**

Richards, Prof. Robert H., Mass. Inst. Tech., Back Bay, Boston, Mass. (22). 1875. **D**

Richards, Mrs. Robert H., Prof. Mass. Inst. of Tech., Back Bay, Boston, Mass. (23). 1878. **C**

Richardson, Clifford, Central Power Station, W. & G. R. R. Co., 14 St. and Penn. Ave., Washington, D. C. (30). 1884. **C**

Ricketts, Prof. Palmer C., 17 1st St., Troy, N. Y. (33). 1887. **D A**

Ricketts, Prof. Pierre de Peyster, 104 John St., New York, N. Y. (26). 1880. **C D E**

RILEY, PROF. C. V., U. S. Entomologist, U. S. National Museum, Washington, D. C. (17). 1874. **F H I**

Risteen, Allen D., Hartford, Conn. (38). 1890. **A B D**

Ritchie, E. S., Newton Highlands, Mass. (10). 1877. **B**

Ritter, W. F. McK., P. O. Box 50, Milton, Pa. (40). 1893.

Roberts, Prof. Isaac P., Ithaca, N. Y. (33). 1886. **I**

Robinson, Benjamin Lincoln, Curator Harvard Herbarium, Cambridge, Mass. (41). 1893. **G**

Robinson, Prof. Franklin C., Bowdoin College, Brunswick, Me. (29). 1889. **C D**

Robinson, Prof. S. W., 1353 Highland St., Columbus, Ohio (30). 1883. **D B A**

Rockwell, Gen. Alfred P., Manchester, Mass. (10). 1882. **E**

Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883. **A D**

Rockwood, Prof. Charles G., jr., College of New Jersey, Princeton, N. J. (20). 1874. **A E B D**

Rogers, Prof. W. A., Colby Univ., Waterville, Me. (15). 1875. **A B D**

Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879. **E**

Rood, Prof. O. N., Columbia College, New York, N. Y. (14). 1875. **B**

Rosa, Edward Bennett, Prof. of Physics, Wesleyan Univ., Middletown, Conn. (39). 1892. **A B**

Ross, Waldo O., 1 Chestnut St., Boston, Mass. (29). 1882.

Rowland, Prof. Henry A., Baltimore, Md. (29). 1880. **B**

Runkle, Prof. J. D., Mass. Institute of Technology, Boston, Mass. (2). 1875. **A D**

Rusby, Henry H., M.D., College of Pharmacy, 211 E. 23d St., New York, N. Y. (36). 1890. **G**

Russell, I. C., Univ. of Mich., Ann Arbor, Mich. (25). 1882. **E**

Ryan, Harris J., Cornell Univ., Ithaca, N. Y. (38). 1890. **B**

Sadtler, Sam'l P., Ph.D., 1042 Drexel Building, Philadelphia, Pa. (22). 1875. **C**

Saegmuller, G. N., 132 Maryland Ave., S. W., Washington, D. C. (38) 1891. **A B**

Safford, Dr. James M., Nashville, Tenn. (6). 1875. **E C F**

Safford, Prof. Truman H., Williamstown, Mass. (41). 1892. **A**

Salisbury, Prof. R. D., Chicago Univ., Chicago, Ill. (37). 1890. **B E**

Salmon, Daniel E., Dep't of Agric., Washington, D. C. (31). 1885. **F**

Sampson, Commander W. T., U. S. N., Navy Dept., Washington, D. C. (25). 1881. **B A**

Saunders, William, Director Canadian Experimental Farms, Ottawa, Ontario, Can. (17). 1874. **F**

Saville, Marshall H., Peabody Museum, Cambridge, Mass. (39). 1892. **H**

SCHAEBERLE, J. M., Astronomer in the Lick Observatory, San José, Cal. (34). 1886. **A**

Schanck, Prof. J. Stillwell, Princeton, New Jersey (4). 1882. **C B H**

Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874. **A**

Schweinitz, Dr. E. A. de, Dep't of Agriculture, Washington, D. C. (36). 1889. **C**

Schweitzer, Prof. Paul, State University of Missouri, Columbia, Mo. (24). 1877. **C B**

Scovell, M. A., Director Kentucky Agricultural Experiment Station, Lexington, Ky. (35). 1887.

Scribner, F. Lamson, Director Tenn. Agricultural Exper. Station, Knoxville, Tenn. (34). 1887. **G**

SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874. **F**

Seaman, W. H.; Chemist, 1424 11th St. N. W., Washington, D. C. (23). 1874. **C F**

Searle, Prof. Geo. M., Catholic Univ., Washington, D. C. (39). 1891. **A**

See, Horace, 1 Broadway, New York, N. Y. (34). 1886. **D**

Seiler, Carl, M.D., 1846 Spruce St., Philadelphia, Pa. (29). 1882. **F B**

Seymour, Arthur Bliss, Cambridge, Mass. (36). 1890. **G**

Sharples, Stephen P., 13 Broad St., Boston, Mass. (29). 1884. **C**

Shelton, Prof. Edward M., Dep't of Agric., Brisbane, Queensland, Australia (32). 1892. **F**

Shimer, Porter W., E.M., Easton, Pa. (38). 1889. **C**

Shufeldt, Dr. R. W., Smithsonian Institution, Washington, D. C. (40). 1892. **F**

Shutt, Frank T., M.A., F.E.C., F.C.S., Chief Chemist Canadian Experimental Farm, Ottawa, Ontario, Can. (38). 1889. **C**

Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.

Silliman, Prof. Justus M., Lafayette Coll., Easton, Pa. (19). 1874. **D E**

Skinner, Aaron N., U. S. Naval Observ., Washington, D. C. (40). 1893. **A**

Skinner, Joseph J., Massachusetts Inst. Technology, Boston, Mass. (23). 1880. **B**

Smiley, Charles W., U. S. Fish Commission, Washington, D. C. (28). 1883. **I**

Smith, Alex., Ph.D., Wabash College, Crawfordsville, Ind. (40). 1892. **C**

Smith, Prof. Chas. J., 35 Adelbert St., Cleveland, Ohio (32). 1885. **A B**

Smith, Prof. Edgar F., Univ. of Penn., Philadelphia, Pa. (33). 1891. **C**

Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30). 1882. **A B**

Smith, Prof. Erastus G., Beloit College, Beloit, Wis. (34). 1887. **C**

Smith, Erwin F., Dep't of Agric., Washington, D. C. (34). 1890. **G**

Smith, Prof. Eugene A., University, Ala. (20). 1877. **E C**

Smith, John B., Professor of Entomology, Rutgers College, New Brunswick, N. J. (32). 1884. **F**

SMITH, QUINTIUS C., M.D., No. 617 Colo. St., Austin, Texas (26). 1881. **F**

Smith, Dr. Theobald, Bureau of Animal Industry, U. S. Dep't of Agric., Washington, D. C. (35). 1887. **F**

Smock, Prof. John Conover, Trenton, N. J. (23). 1879. **E**

Snow, Prof. Benj. W., Bloomington, Ind. (35). 1889. **B**

Snow, Prof. F. H., Lawrence, Kan. (29). 1881. **F E**

Snow, Julia W., La Salle, Ill. (39). 1892. **F**

Snyder, Henry, B.Sc., Miami Univ., Oxford, Ohio (30). 1888. **B C**

Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa (24). 1882. **A B**

Soule, R. H., Roanoke, Va. (33). 1886. **D**

Spalding, Volney M., Ann Arbor, Mich. (34). 1886. **G**

Spencer, Guilford L., Department Agriculture, Washington, D. C. (36). 1889. **C D**

Spencer, Prof. J. William, 7 Church St., Atlanta, Ga. (28). 1882. **E**

Springer, Dr. Alfred, Box 621, Cincinnati, Ohio (24). 1880. **C**

Staley, Cady, LL.D., Pres. Case School of Applied Sciences, Cleveland, Ohio (37). 1888. **D**

Starr, Frederick, Ph.D., Prof. Univ. of Chicago, Chicago, Ill. (36). 1892. **H E**

Stearns, R. E. C., care Smithsonian Institution, Washington, D. C. (18). 1874. **F**

Stedman, Prof. John M., A. and M. Coll., Auburn, Ala. (40). 1892. **F**

Stejneger, Leonhard, Curator Dept. of Reptiles, National Museum, Washington, D. C. (40). 1892. **F**

STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874. **E H**

Sternberg, Col. George M., Surgeon U. S. A., Army Building, 39 Whitehall St., New York, N. Y. (24). 1880. **F**

Stevens, Prof. W. LeConte, Rensselaer Polytechnic Inst., Troy, N. Y. (29). 1882. **B A C**

Stevenson, Prof. John J., Univ. of New York, New York, N. Y. (36). 1888.

Stevenson, Mrs. Matilda C., Bureau of Ethnology, Washington, D. C. (41). 1893. **H**

Stiles, Dr. Chas. W., Dept. of Agric., Washington, D. C. (40). 1892. **F**
Stoddard, Prof. John T., Smith College, Northampton, Mass. (35). 1889. **B C**
Stokes, Henry Newlin, Ph.D., Univ. of Chicago, Chicago, Ill. (38). 1891. **C E**
Stone, Ormond, Director Leander McCormick Observatory, University of Virginia, Va. (24). 1876. **A**
Stone, Prof. Winthrop E., Purdue Univ., La Fayette, Ind. (39). 1891. **C**
Story, Prof. Wm E., Clark Univ., Worcester, Mass. (29). 1881. **A**
Stowell, Prof. T. B., Potsdam, N. Y. (28). 1885. **F**
Stringham, Prof. Irving, Univ. of Cal., Berkeley, Cal. (33). 1885. **A**
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874. **C**
Sturgis, Wm. C., 384 Whitney Ave., New Haven, Conn. (40). 1892. **G**
Sturtevant, E. Lewis, M.D., So. Framingham, Mass. (29). 1882. **G**
Swift, Lewis, Ph.D., Warner Observatory, Rochester, N. Y. (29). 1882. **A**
Swingle, W. T., Eustis, Florida (40). 1892. **G**

Tainter, Charles Sumner, 1843 S, cor. 19 St., Washington, D. C. (29). 1881. **B D A**
Taylor, H. C., Commander U. S. N. (30). 1889.
Taylor, Thos., M.D., Department of Agriculture, Washington, D. C. (29). 1885. **F C**
Taylor, William B., Smithsonian Institution, Washington, D. C. (29). 1881. **B A**
Terry, Prof. N. M., U. S. Naval Acad., Annapolis, Md. (23). 1874. **B**
Thomas, Benj. F., Ph.D., State Univ., Columbus, Ohio (29). 1882. **B A**
Thompson, Joseph Osgood, Haverford College, Pa. (41). 1893.
Thomson, Elihu, Thomson-Houston Electric Co., Lynn, Mass. (37). 1888. **B**
Thomson, Wm., M.D., 1426 Walnut St., Philadelphia, Pa. (33). 1885. **B**
Thruston, Gates Phillips, Nashville, Tenn. (38). 1890. **H**
Thurston, Prof. R. H., Sibley College, Cornell University, Ithaca, N. Y. (23). 1875. **D**
Thwing, Charles B., Northwestern Univ., Evanston, Ill. (38). 1892. **B**
Tittmann, Otto H., U. S. Coast and Geodetic Survey Office, Washington, D. C. (24). 1888. **A**
Todd, Prof. James E., Vermillion, So. Dak. (22). 1886. **E F**
Townshend, Prof. N. S., Ohio State Univ., Columbus, Ohio (17). 1881. **F H**
Tracy, Sam'l M., Agricultural College, Miss. (27). 1881. **G**
Traphagen, Frank W., Ph.D., Prof. of Chem., The College of Montana, Deer Lodge City, Montana (35). 1889. **C F E**
Trelease, Dr. Wm., Director Missouri Botanical Gardens, St. Louis, Mo. (39). 1891. **G**
Trimble, Prof. Henry, 145 No. 10 St., Philadelphia, Pa. (34). 1889. **C**
True, Fred W., U. S. National Museum, Washington, D. C. (28). 1882. **F**
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882. **H**

Tucker, Willis G., M.D., Albany Med. Coll., Albany, N. Y. (29). 1888. **C**
TUCKERMAN, ALFRED, Ph.D., 342 W. 57th St., New York, N. Y. (39). 1891. **C**
Tuttle, Prof. Albert H., Univ. of Virginia, Charlottesville, Va. (17). 1874. **F**
Twitchell, E., 10 Bellevue Ave., Mt. Auburn, Cincinnati, Ohio (39). 1891. **C**

Uhler, Philip R., 254 W. Hoffman St., Baltimore, Md. (19). 1874. **F E**
Underwood, Prof. Lucien M., De Pauw Univ., Greencastle, Ind. (33). 1885. **G**
Upham, Warren, 124 State St., Minneapolis, Minn. (25). 1880. **E**
Upton, Winslow, Brown Univ., Providence, R. I. (29). 1883. **A**

Van der Weyde, P. H., M.D., 236 Duffield St., Brooklyn, N. Y. (17). 1874. **B**
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882. **B C F**
Van Hise, Charles R., Univ. of Wisconsin, Madison, Wis. (37). 1890.
Van Vleck, Prof. John M., Wesleyan Univ., Middletown, Conn. (23). 1875. **A**
Venable, Prof. F. P., Chapel Hill, N. C. (39). 1891. **C**
Very, Samuel W., Lieut. Comdr. U. S. N., Allerton St., near High, Brookline, Mass. (28). 1886. **A B**
Vining, Edward P., care Chas. B. Griggs, Washington St., Brookline, Mass. (32). 1887. **H**
Vogdes, A. W., Capt. 5th Art'y, Alcatraz Island, San Francisco, Cal. (32). 1885. **E F**

Wachsmuth, Charles, 111 Marietta St., Burlington, Iowa (30). 1884. **E F**
Wadsworth, Prof. M. Edward, Ph.D., Director of the Michigan Mining School, State Geologist of Michigan, Houghton, Mich. (23). 1874. **E**
Walcott, Charles D., U. S. Geological Survey, Washington, D. C. (25). 1882. **E F**
Waldo, Prof., Clarence A., Greencastle, Ind. (37). 1889. **A**
Waldo, Leonard, S. D., Bridgeport, Conn. (28). 1880. **A**
Wallace, Wm., Ansonia, Conn. (28). 1882.
WALLER, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walmsley, W. H., 69 Washington St., Chicago, Ill. (28). 1883. **F**
Wanner, Atreus, York, York Co., Pa. (36). 1890. **H**
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875. **F E H**
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E G**
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874. **G F**
Ward, Wm. E., Port Chester, N. Y. (36). 1889. **D**
Warder, Prof. Robert B., Howard Univ., Washington, D. C. (19). 1881. **C B**

Warner, Prof. A. G., Leland Stanford jr. Univ., Palo Alto, Cal. (38). 1892. **I**

WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 1874. **A B**

Warner, Worcester R., 887 Case Ave., Cleveland, Ohio (33). 1888. **A B D**

Warren, Cyrus M., Brookline, Mass. (29). 1882. **C**

Warren, Dr. Joseph W., Bryn Mawr Col., Bryn Mawr, Pa. (31). 1886. **F**

Warren, Prof. S. Edward, Newton, Mass. (17). 1875. **A–I**

WATSON, PROF WM., 107 Marlborough St., Boston, Mass. (12). 1884. **A**

Webb, Prof. J. Burkitt, Stevens Inst., Hoboken, N. J. (31). 1883. **D B A**

Weber, Prof. Henry A., Ohio State Univ., Columbus, Ohio (35). 1888. **F**

Webster, F. M., Wooster, Ohio (35). 1890. **F**

Webster, Prof. N. B., Grove House, Vineland, N. J. (7). 1874. **B C E**

Weed, Clarence M., Hanover, N. H. (38). 1890.

Wheeler, Prof. C. Gilbert, 143 Lake St., Chicago, Ill. (18). 1883. **C E**

Wheeler, Orlando B., Office Mo. River Com., 1515 Lucas Place, St. Louis, Mo. (24). 1882. **A D**

White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875. **E F**

White, David, U. S. National Museum, Washington, D. C. (40). 1892. **E F**

White, Prof. H. C., Univ. of Georgia, Athens, Ga. (29). 1885. **C**

WHITE, PROF. I. C., Univ. of W. Va., Morgantown, W. Va. (25). 1882. **E**

Whiteaves, J. F., Geol. Survey, Ottawa, Ontario, Can. (31). 1887. **E F**

Whitfield, R. P., American Museum Natural History, 77th St. and 8th Avenue, New York, N. Y. (18). 1874. **E F H**

Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883. **B A**

Whitman, Prof. Frank P., Adelbert College, Cleveland, Ohio (33). 1885. **A B**

Wilbur, A. B., Middletown, N. Y. (23). 1874.

Wiley, Prof. Harvey W., Dep't of Agric., Washington, D.C. (21). 1874. **C**

Williams, Benezette, 171 La Salle St., Chicago, Ill. (33). 1887. **D**

Williams, Charles H., M.D., C. B. and Q. Gen. Office, Adams St., Chicago, Ill. (22). 1874.

Williams, Geo. Huntington, Johns Hopkins Univ., Baltimore, Md. (33). 1886. **E**

Williams, Prof. Henry Shaler, Yale College, New Haven, Conn. (18). 1882. **E F**

Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874. **H F**

Williams, Prof. S. G., Cornell Univ., Ithaca, N. Y. (33). 1885. **E**

Willis, Bailey, U. S. Geol. Survey, Washington, D. C. (36). 1890.

Willmott, Arthur B., 6 Little's Block, Cambridge, Mass. (38). 1890.

Willson, Prof. Frederick N., Princeton, N. J. (33). 1887. **A D**

Willson, Robert W., Cambridge, Mass. (30). 1890. **B A**

Wilson, Herbert M., U. S. Geol. Survey, Washington, D. C. (40). 1892. **D E**

Wilson, Joseph M., Room 1036, Drexel Building, Philadelphia, Pa. (33). 1886. **D**

Wilson, Thomas, U. S. Nat'l Museum, Washington, D. C. (36). 1888. **H**

Wilson, Prof. William Powell, Dept. of Biology, Univ. of Pa., Philadelphia, Pa. (38). 1889. **G**

Winchell, Horace V., 1306 S. E. 7th St., Minneapolis, Minn. (34). 1890. **E C**

Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874. **E H**

Wing, Henry H., 3 Reservoir Ave., Ithaca, N. Y. (38). 1890.

Winlock, Wm. C., Smithsonian Institution, Washington, D. C. (33). 1885. **A B**

Winslow, Arthur, State Geologist, Jefferson City, Mo. (37). 1889. **E**

Winterhalter, A. G., Geologist and Mining Expert, Rooms 411 and 412 Roe Building, 5th and Pine Sts., St. Louis, Mo. (37). 1893. **A**

Withers, Prof. W. A., Agric. and Mechanical College, Raleigh, N. C. (33). 1891. **C**

Witthaus, Dr. R. A., 410 E. 26th St., New York, N. Y. (35). 1890.

Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.

Woodbury, C. J. H., 31 Milk St., Boston, Mass. (29). 1884. **D**

Woodward, Prof. Calvin M., 1761 Missouri Ave., St. Louis, Mo. (32). 1884. **D A I**

Woodward, R. S., Columbia College, New York, N. Y. (33). 1885. **A B D**

Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.

Worthen, W. E., 63 Bleeker St., New York, N. Y. (36). 1888. **D**

Wrampelmeier, Theo. J., Room 17, Appraiser's Building, San Francisco, Cal. (34). 1887. **C**

Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880. **E F**

Wright, Prof. Arthur W., Yale Coll., New Haven. Conn. (14). 1874. **A B**

Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882. **E**

Wright, Prof. Thos. W., Union College, Schenectady, N. Y. (36). 1889.

Würtele, Rev. Louis C., Acton Vale, P. Q., Can. (11). 1875. **E**

Youmans, Wm. Jay, M.D., Popular Science Monthly, 1–5 Bond St., New York, N. Y. (28). 1889. **F C**

Young, A. V. E., Northwestern Univ., Evanston, Ill. (33). 1886. **C B**

Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874. **A B D**

Zalinski, E. L., Capt. 5th Artillery, U. S. A., Fort Hamilton, New York Harbor, N. Y. (36). 1891. **D**

Ziwet, Alexander, 14 So. State St., Ann Arbor, Mich. (38). 1890. **A**

[797 FELLOWS.]

SUMMARY.—PATRONS, 2; CORRESPONDING MEMBERS, 2; MEMBERS, 1138; HONORARY FELLOWS, 1; FELLOWS, 796.

APRIL 30, 1894, TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1939.

# DECEASED MEMBERS.

[Unless by special vote of the Council, the names of those only who are members of the Association at the *time of their decease* will be included in this list. Information of the date and place of birth and death, to fill blanks in this list, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.

Abert, John James, Washington, D. C. (1). Born in Shepherdstown, Va., Sept. 17, 1788. Died in Washington, D. C., Sept. 27, 1863.

Adams, Charles Baker, Amherst, Mass. (1). Born in Dorchester, Mass., Jan. 11, 1814. Died in St. Thomas, W. I., Jan. 19, 1853.

Adams, Edwin F., Charlestown, Mass. (18).

Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.

Agassiz, Louis, Cambridge, Mass. (1). Born in Parish of Motier, Switzerland, May 28, 1807. Died in Cambridge, Mass., Dec. 14, 1873.

Ainsworth, J. G., Barre, Mass. (14).

Alexander, Stephen, Princeton, N. J. (1). Born Sept. 1, 1806. Died June 25, 1883.

Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.

Allen, Zachariah, Providence, R. I. (1). Born in Providence, R. I., Sept. 15, 1795. Died March 17, 1882.

Allston, Robert Francis Withers, Georgetown, S. C. (3). Born in All Saints Parish, S. C., April 21, 1801. Died near Georgetown, S. C., April 7, 1864.

Alvord, Benjamin, Washington, D. C. (17). Born in Rutland, Vt., Aug. 18, 1813. Died Oct. 16, 1884.

Ames, Nathan P., Springfield, Mass. (1). Born in 1803. Died Apr. 23, 1847.

Andrews, Ebenezer Baldwin, Lancaster, Ohio (7). Born in Danbury, Conn., April 29, 1821. Died in Lancaster, Ohio, Aug. 14, 1880.

Anthony, Charles H., Albany, N. Y. (6). Died in 1874.

Antisell, Thomas, Washington, D. C. (33).

Appleton, Nathan, Boston, Mass. (1). Born in New Ipswich, N. H., Oct. 6, 1779. Died July 14, 1861.

Armstrong, John W., Fredonia, N. Y. (24).

Ashburner, Charles A., Pittsburgh, Pa. (31). Died Dec. 24, 1889.

Ashburner, Wm., San Francisco, Cal. (29). Born in Stockbridge, Mass., March, 1831. Died in San Francisco, Cal., April 20, 1887.

Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11, 1878.

Aufrecht, Louis, Cincinnati, Ohio (30).

Baba, Tatui, New York, N. Y. (36).

Babbitt, Miss Franc E., Coldwater, Mich. (32). Died near Coldwater, Mich., July 6, 1891, aged 67.

Bache, Alexander Dallas, Washington, D. C. (1). Born in Philadelphia, Pa., July 19, 1806. Died at Newport, R. I., Feb. 17, 1867.

Bache, Franklin, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Oct. 25, 1792. Died March 19, 1864.

Bailey, Jacob Whitman, West Point, N. Y. (1). Born in Auburn, Mass., April 29, 1811. Died in West Point, N. Y., Feb. 26, 1857.

Baird, Spencer Fullerton, Washington, D. C. (1). Born in Reading, Pa., Feb. 3, 1823. Died in Wood's Holl, Mass., Aug. 19, 1887.

Bardwell, F. W., Lawrence, Kan. (13). Died in 1878.

Barnard, F. A. P., New York, N. Y. (7). Born in Sheffield, Mass., May 5, 1809. Died in New York, April 27, 1889.

Barnard, John Gross, New York, N. Y. (14). Born in Sheffield, Mass., May 19, 1815. Died in Detroit, Mich., May 14, 1882.

Barrett, Dwight H., Baltimore, Md. (36). Died in March, 1889.

Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.

Barry, Redmond, Melbourne, Australia (25).

Bassett, Daniel A., Los Angeles, Cal. (29). Born Dec. 8, 1819. Died May 26, 1887.

Bassnett, Thomas, Jacksonville, Fla. (8). Born 1807. Died in Jacksonville, Fla., Feb. 16, 1886.

Batchelder, John Montgomery, Cambridge, Mass. (8). Born in New Ipswich, N. H., Oct. 13, 1811. Died in Cambridge, July 3, 1892.

Bayne, Herbert Andrew, Kingston, Ont., Can. (29). Born in Londonderry, Nova Scotia, Aug. 16, 1846. Died in Pictou, Can., Sept. 16, 1886.

Beach, J. Watson, Hartford, Conn. (23). Born Dec. 28, 1823. Died Mar. 16, 1887.

Beauregard, Gustave T., New Orleans, La. (30). Died Feb. 20, 1893, aged 75.

Beck, C. F., Philadelphia, Pa. (1).

Beck, Lewis Caleb, New Brunswick, N. J. (1). Born in Schenectady, N. Y., Oct. 4, 1798. Died April 20, 1853.

Beck, Theodoric Romeyn, Albany, N. Y. (1). Born in Schenectady, N. Y., Aug. 11, 1791. Died in Utica, N. Y., Nov. 19, 1855.

Beckwith, Henry C., Coleman's Station, N. Y. (29). Died July 12, 1885.

Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.

Belknap, William B., Louisville, Ky. (29).

Bell, Samuel N., Manchester, N. H. (7). Born in Chester, N. H., March 25, 1829. Died in Manchester, N. H., Feb. 8, 1889.

Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.

Benedict, George Wyllys, Burlington, Vt. (16). Born Jan. 11, 1796. Died Sept. 23, 1871.

Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19, 1877.

Binney, Amos, Boston, Mass. (1). Born in Boston, Mass., Oct. 18, 1803. Died in Rome, Feb. 18, 1847.

Binney, John, Boston, Mass. (3).

Blackie, Geo. S., Nashville, Tenn. (26).

Blair, Henry W., Washington, D. C. (26). Died Dec. 15, 1884.

Blake, Eli Whitney, New Haven, Conn. (1). Born Jan. 27, 1795. Died Aug. 18, 1886.

Blake, Francis C., Mansfield Valley, Pa. (29). Died Feb. 21, 1891.

Blake, Homer Crane, New York, N. Y.. (28). Born in Cleveland, Ohio, Feb. 1, 1822. Died in New York, N. Y., Jan. 20, 1880.

Blanding, William, ——, R. I. (1).

Blatchford, Thomas W., Troy, N. Y. (6).

Blatchley, Miss S. L., New Haven, Conn. (19). Died March 13, 1873.

Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.

Bomford, George, Washington, D. C. (1). Born in New York, N. Y., 1780. Died in Boston, Mass., March 25, 1848.

Bowditch, Henry Ingersoll, Boston, Mass. (2). Born in Salem, Mass., Aug. 9, 1808. Died in Boston, Mass., Jan. 14, 1892.

Bowles, Miss Margaretta, Columbia, Tenn. (26). Died July, 1887.

Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.

Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.

Braithwaite, Jos., Chambly, C. W. (11).

Breckinridge, S. M., St. Louis, Mo. (27). Died May 28, 1891.

Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.

Briggs, Robert, Philadelphia, Pa. (29). Born May 18, 1822. Died July 24, 1882.

Brigham, Charles Henry, Ann Arbor, Mich. (17). Born in Boston, Mass., July 27, 1820. Died Feb. 19, 1879.

Bross, William, Chicago, Ill. (7). Died in 1890.

Brown, Andrew, Natchez, Miss. (1).

Brown, Horace, Salem, Mass. (27). Died in July, 1883.

Bull, John, Washington, D. C. (31). Born Aug. 1, 1819. Died June 9, 1884.

Bulloch, Walter H., Chicago, Ill. (30).

Burbank, L. S., Woburn, Mass. (18).

Burgess, Edward, Boston, Mass. (22). Born in Barnstable, Mass., June 30, 1848. Died in Boston, July 12, 1891.

Burke, Joseph Chester, Middletown, Conn. (29). Died in 1885.

Burnap, George Washington, Baltimore, Md. (12). Born in Merrimack, N. H., Nov. 30, 1802. Died in Philadelphia, Pa., Sept. 8, 1859.

Burnett, Waldo Irving, Boston, Mass. (1). Born in Southborough, Mass., July 12, 1828. Died in Boston, Mass., July 1, 1854.

Butler, Thomas Belden, Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, Frederick A., New York, N. Y. (27). Died in 1879.

Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.

Carpenter, Thornton, Camden, S. C. (7).

Carpenter, William M., New Orleans, La. (1).

Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5, 1880.

Case, William, Cleveland, Ohio (6).

Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died in Providence, R. I., Jan. 8, 1877.

Chadbourne, Paul Ansel, Amherst, Mass. (10). Born in North Berwick, Me., Oct. 21, 1823. Died Feb. 23, 1883.

Chapin, J. H., Meriden, Conn. (33). Died in 1892.

Chapman, Nathaniel, Philadelphia, Pa. (1). Born in Alexandria Co., Va., May 28, 1780. Died July 1, 1853.

Chase, Pliny Earle, Haverford College, Pa. (18). Born in Worcester, Mass., Aug. 18, 1820.

Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.

Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec. 13, 1870.

Cheesman, Louis Montgomery, Hartford, Conn. (32). Born in 1858. Died in Jan., 1885.

Cheney, Miss Margaret S., Jamaica Plain, Mass. (29). Died in 1882.

Chevreul, Michel Eugène, Paris, France (35). Born in Angiers, France, Aug. 31, 1786. Died April 9, 1889.

Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.

Clark, Henry James, Cambridge, Mass. (13). Born in Easton, Mass. June 22, 1826. Died in Amherst, Mass., July 1, 1873.

Clark, Joseph, Cincinnati, Ohio (5).

Clark, Patrick, Rahway, N. J. (33). Died March 5, 1887.

Clarke, A. B., Holyoke, Mass. (13).

Clarke, Charles S., Peoria, Ill. (34). Died Nov. 15, 1890.

Cleaveland, C. H., Cincinnati, Ohio (9).

Cleveland, A. B., Cambridge, Mass. (2).

Coffin, James Henry, Easton, Pa. (1). Born in Northampton, Mass., Sept. 6, 1806. Died Feb. 6, 1873.

Coffin, John H. C., Washington, D. C. (1). Born in Wiscasset, Maine, Sept. 14, 1815. Died in Washington, D. C., Jan. 8, 1890.

Coffinberry, Wright Lewis, Grand Rapids, Mich. (20). Born in Lancaster, Ohio, April 5, 1807. Died in Grand Rapids, Mich., March 26, 1889.

Colburn, E. M., Peoria, Ill. (33). Born in Rome, N. Y., Sept. 13, 1813. Died in Peoria, Ill., May 29, 1890.

Cole, Frederick, Montreal, Can. (31). Died in 1887.

Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.

Coleman, Henry, Boston, Mass. (1).

Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.

Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in New Jersey, June 21, 1803. Died Aug. 9, 1877.

Cook, George H., New Brunswick, N. J. (4). Born in Hanover, Morris County, in 1818. Died in New Brunswick, N. J., Sept. 22, 1889.

Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.

Cooper, William, Hoboken, N. J. (9). Died in 1864.

Cope, Mary S., Germantown, Pa. (33). Born in Germantown, Pa., July 13, 1853. Died in Germantown, Jan. 4, 1888.

Copes, Joseph S., New Orleans, La. (11). Born Dec. 9, 1811. Died March 1, 1885.

Corning, Erastus, Albany, N. Y. (6). Born in Norwich, Conn., Dec. 14, 1794. Died April 9, 1872.

Costin, M. P., Fordham, N. Y. (30). Died June 8, 1884.

Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.

Coyrière-Pardo, E. Miriam, New York, N. Y. (36). Born in London, Eng., Sept. 2, 1845. Died in New York, N. Y., Feb. 6, 1893.

Cramp, John Mockett, Wolfville, N. S. (11). Born in Kent, England, July 25, 1796. Died Dec. 6, 1881.

Crehore, John D., Cleveland, Ohio (24).

Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.

Crocker, Miss Lucretia, Boston, Mass. (29). Died in 1886.

Crosby, Alpheus, Salem, Mass. (10). Born in Sandwich, N. H., Oct. 13, 1810. Died April 17, 1874.

Crosby, Thomas Russell, Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.

Crosier, Edward S., New Albany, Ind. (29). Died in June, 1891.

Croswell, Edwin, Albany, N. Y. (6). Born in Catskill, N. Y., May 29, 1797. Died June 13, 1871.

Crow, Wayman, St. Louis, Mo. (27). Born March 7, 1808. Died May 10, 1885.

Cummings, Joseph, Evanston, Ill. (13). Born in Falmouth, Me., March 3, 1817. Died in Evanston, Ill., May 7, 1890.

Curry, W. F., Geneva, N. Y. (11).

Curtis, George William, Staten Island, N. Y. (36).

Curtis, Josiah, Washington, D. C. (18). Died Aug. 1, 1883.

Cutting, Hiram Adolphus, Lunenburgh, Vt. (17). Born in Concord, Vt., Dec. 23, 1832. Died in Lunenburgh, April 18, 1892.

Da Costa, Chas. M., New York, N. Y. (36). Died in 1890.

Dalrymple, Edwin Augustine, Baltimore, Md. (11). Born in Baltimore, Md., June 4, 1817. Died Oct. 30, 1881.

Danforth, Edward, Elmira, N. Y. (11). Died in Elmira, N. Y., June 13, 1888.

Davenport, H. W., Washington, D. C. (30).

Davis, I. Thomas, Washington, D. C. (40). Died Jan. 19, 1892.

Day, Austin G., New York, N. Y. (29). Died Dec. 28, 1889.

Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.

Dean, Amos, Albany, N. Y. (6). Born in Barnard, Vt., Jan. 16, 1803. Died Jan. 26, 1868.

Dearborn, George H. A. S., Roxbury, Mass. (1).

Dekay, James Ellsworth, New York, N. Y. (1). Born in New York, 1792, Died Nov. 21, 1851.

Delano, Joseph C., New Bedford, Mass. (5). Born Jan. 9, 1796. Died Oct. 16, 1886.

De Laski, John, Carver's Harbor, Me. (18).

Devereux, John Henry, Cleveland, Ohio (18). Born in Boston, Mass., April 5, 1832. Died in Cleveland, Ohio, March 17, 1886.

Dewey, Chester, Rochester, N. Y. (1). Born in Sheffield, Mass., Oct. 25, 1781. Died Dec. 15, 1867.

Dexter, G. M., Boston, Mass. (11).

Dickerson, Edward N., New York, N. Y. (36).

Dillingham, W. A. P., Augusta, Me. (17).

Dimmick, L. N., Santa Barbara, Cal. (29). Died May 31, 1884.

Dinwiddie, Hardaway H., College Station, Texas (32). Died Dec. 11, 1887.

Dinwiddie, Robert, New York, N. Y. (1). Born in Dumfries, Scotland, July 23, 1811. Died in New York, N. Y., July 12, 1888.

Dixwell, Geo. B., Boston, Mass. (29). Died April, 1885.

Doggett, George Newell, Chicago, Ill. (33). Born in Chicago, Ill., Dec. 19, 1858. Died in Fredericksburg, Va., Jan. 15. 1887.

Doggett, Mrs. Kate Newell, Chicago, Ill. (17). Born in Castleton, Vt., Nov. 5, 1828. Died in Havana, Cuba, March 13, 1884.

Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.

Doolittle, L., Lenoxville, C. E. (11). Died in 1862.

Dorand, Fred James, Chester, Vt. (38). Born in Rockingham, Vt., Dec. 6, 1856. Died in Aiken, S. C., April 17, 1893.

Dorr, Ebenezer Pearson, Buffalo, N. Y. (25). Born in Hartford, Vt. Died in Buffalo, N. Y., April 29, 1882.

Dow, John Melmoth, New York, N. Y. (31). Died in New York, Nov. 4, 1892.

Dowling, John W., New York, N. Y. (36). Born in New York, Aug. 15, 1837. Died in Goshen, N. Y., Jan. 15, 1892.

Draper, Henry, New York, N. Y. (28). Born in New York, N. Y., March 7, 1837. Died Nov. 20, 1882.

Drowne, Charles, Canaan Four Corners, N. Y. (6). Died in 1888.

Ducatel, Julius Timoleon, Baltimore, Md. (1). Born in Baltimore, Md., June 6, 1798. Died April 25, 1849.

Duffield, George, Detroit, Mich. (10). Born in Strasburg, Pa., July 4, 1794. Died in Detroit, Mich., June 26, 1869.

Dumont, A. H., Newport, R. I. (14).

Dun, Walter Angus, Cincinnati, Ohio (31). Born March 1, 1857. Died Nov. 7, 1887.

Duncan, Lucius C., New Orleans, La. (10). Born in 1801. Died Aug. 9, 1855.

Dunn, Robinson P., Providence, R. I. (14). Born in Newport, R. I., May 31, 1825. Died in Newport, Aug. 28, 1867.

Dury, Henry M., Nashville, Tenn. (33). Died April 15, 1891.

Eads, James Buchanan, New York, N. Y. (27). Born May 23, 1820. Died March 8, 1887.

Easton, Norman, Fall River, Mass. (14). Died Dec. 21, 1872.

Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.

Elliott, Ezekiel Brown. Washington, D. C. (10). Born July 16, 1823. Died May 24, 1888.

Elsberg, Louis, New York, N. Y. (23). Born in Iserlohn, Prussia, April 2, 1836. Died in New York, N. Y., Feb. 19, 1885.

Elwyn, Alfred Langdon, Philadelphia, Pa. (1). Born in Portsmouth, N. H., July 9, 1804. Died in Philadelphia, Pa., March 15, 1884.

Ely, Charles Arthur, Elyria, Ohio (4).

Emerson, Geo. Barrell, Boston, Mass. (1). Born in Kennebunk, Me., Sept. 12, 1797. Died March 14, 1881.

Emmons, Ebenezer, Williamstown, Mass. (1). Born in Middlefield, Mass., May 16, 1799. Died Oct. 1, 1863.

Engelmann, George, St. Louis, Mo. (1). Born in Frankfort-on-the Main, Germany, Feb. 2, 1809. Died Feb. 4, 1884.

Engstrom, A. B., Burlington, N. J. (1).

Eustis, Henry Lawrence, Cambridge, Mass. (2). Born Feb. 1, 1819. Died Jan. 11, 1885.

Evans, Asher B., Lockport, N. Y. (19). Born in Hector, N. Y., Sept. 21, 1834. Died in Lockport, Sept. 24, 1891.

Evans, Edwin, Streator, Ill. (30). Died May 5, 1889.

Everett, Edward, Boston, Mass. (2) Born in Dorchester, Mass., April 11, 1794. Died in Boston, Mass., Jan. 15, 1865.

Ewing, Thomas, Lancaster, Ohio (5). Born in Ohio Co., Va., Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.

Farmer, Moses G., Eliot, Me. (9). Died in Chicago, Ill., May 25, 1893.

Farnam, J. E., Georgetown, Ky. (26).

Farquharson, Robert James, Des Moines, Iowa (24). Born July 15, 1824. Died Sept. 6, 1884.

Felton, Samuel Morse, Philadelphia, Pa. (29). Born in Newbury, Mass., July 19, 1809. Died in Philadelphia, Pa., Jan. 24, 1889.

Ferrel, William, Kansas City, Mo. (11). Died Sept. 18, 1891.

Ferris, Isaac, New York, N. Y. (6). Born in New York, Oct. 9, 1798. Died in Roselle, N. J., June 16, 1873.

Feuchtwanger, Lewis, New York, N. Y. (11). Born in Fürth, Bavaria, Jan. 11, 1805. Died in New York, N. Y., June 25, 1876.

Ficklin, Joseph, Columbia, Mo. (20). Born in Winchester, Ky., Sept. 9, 1833. Died in Columbia, Mo., Sept. 6, 1887.

Fillmore, Millard, Buffalo, N. Y. (7). Born in New York, Jan. 7, 1800. Died March 8, 1874.

Fisher, Mark, Trenton, N. J. (10).

Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.

Fitch, O. H., Ashtabula, Ohio (7). Born in 1803. Died Sept. 17, 1882.

Floyd, Richard S., San Francisco, Cal. (34). Died Oct. 17, 1890.

Foote, Herbert Carrington, Cleveland, Ohio (35). Born in 1852. Died in Cleveland, Aug. 24, 1888.

Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born in New Jersey, Nov. 26, 1790. Died in Washington, D. C., Jan. 23, 1868.
Ford, A. C., Nashville, Tenn. (26).
Forshey, Caleb Goldsmith, New Orleans, La. (21). Born in Somerset Co., Pa., July 18, 1812. Died in Carrollton, La., July 25, 1881.
Foster, John Wells, Chicago, Ill. (1). Born in Brimfield, Mass., March 4, 1815. Died in Chicago, Ill., June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. Bentley, Boston, Mass. (1). Born in Boston, Mass., Oct. 17, 1795. Died Feb. 6, 1865.
Fox, Charles, Grosse Ile, Mich. (7).
Fox, Joseph G., Easton, Pa. (31). Born in Adams, N. Y., Sept. 7, 1833. Died in Easton, Pa., Dec. 27, 1889.
Frazer, John Fries, Phila., Pa. (1). Born July 8, 1812. Died Oct. 12, 1872.
Freeman, Spencer Hedden, Cleveland, Ohio (29). Born Oct. 3, 1855. Died Feb. 2, 1886.
French, John William, West Point, N. Y. (11). Born in Connecticut, about 1810. Died in West Point, N. Y., July 8, 1871.
Fristoe, E. T., Washington, D. C. (40).
Frothingham, Frederick, Milton, Mass. (11). Born in Montreal, P. Q., April 9, 1825. Died in Milton, March 19, 1891.
Fuller, H. Weld, Boston, Mass. (29). Died Aug. 14, 1889.

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.
Gardiner, Frederic, Middletown, Conn. (23). Born in Gardiner, Me., Oct. 22, 1822. Died in Middletown, Conn., July 17. 1889.
Garrison, H. D., Chicago, Ill. (31). Died in Feb., 1891.
Gavit, John E., New York, N. Y. (1) Born in New York, Oct. 29, 1819. Died in Stockbridge, Mass., Aug. 25, 1874.
Gay, Martin, Boston, Mass. (1). Born in 1804. Died Jan. 12, 1850.
Genth, Friedrich Augustus, Philadelphia, Pa. (24). Born in Waechtersbach, Hesse Cassel, May 17, 1820. Died in Philadelphia, Pa., Feb. 2, 1892.
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, William Mitchell, Schenectady, N. Y. (10) Born in New York N. Y., 1816. Died in New York, Jan. 1, 1868.
Gilmore, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gordon, William J., Cleveland, Ohio (29). Died Nov. 23, 1892.
Gould, Augustus Addison, Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, Benjamin Apthorp, Boston, Mass. (2). Born in Lancaster, Mass., June 15, 1787. Died Oct. 24, 1859.
Graham, James D., Washington, D. C. (1). Born in Virginia, 1799. Died in Boston, Mass., Dec. 28, 1865.

Gray, Alonzo, Brooklyn, N. Y. (13). Born in Townsend, Vt., Feb. 21, 1808. Died in Brooklyn, N. Y., March 10, 1860.

Gray, Asa, Cambridge, Mass. (1). Born in Paris, N. Y., Nov. 18, 1810. Died in Cambridge, Mass., Jan. 30, 1888.

Gray, James H., Springfield, Mass. (6).

Green, Everett Wilmer, Madison, N. J. (10). Born Oct. 5, 1834. Died in 1864.

Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.

Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.

Greer, James, Dayton, Ohio (20). Died in Feb., 1874.

Griffith, Robert Eglesfield, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Feb 13, 1798. Died June 26, 1854.

Griswold, John Augustus, Troy, N. Y. (19). Born Nov. 11, 1818. Died Oct. 31, 1872.

Guest, William E., Ogdensburg, N. Y. (6).

Guyot, Arnold, Princeton, N. J. (1). Born Sept. 5, 1809. Died Feb. 8, 1884.

Habel, Louis, Northfield, Vt. (34).

Hackley, Charles William, New York, N. Y. (4). Born in Herkimer Co., N. Y., March 9, 1809. Died in New York, N. Y., January 10, 1861.

Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.

Hagen, Hermann A., Cambridge, Mass. (17). Born in Konigsberg, Prussia, May 30, 1817. Died in Cambridge, Nov. 9, 1893.

Haldeman, Samuel Stehman, Chickies, Pa. (1). Born Aug. 12, 1812. Died Sept. 10, 1880.

Hale, Enoch, Boston, Mass. (1). Born in Westhampton, Mass., Jan. 29, 1790. Died in Boston, Mass., Nov. 12, 1848.

Hamilton, Jno. M., Coudersport, Pa. (33).

Hampson. Thomas, Washington, D. C. (33).

Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.

Harding, Myron H., Lawrenceburg, Ind. (30). Died Sept., 1885.

Hare, Robert, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Jan. 17, 1781. Died in Philadelphia, May 15, 1858.

Harger, Oscar, New Haven, Conn. (25). Born in Oxford, Conn., Jan. 12, 1843. Died in New Haven, Conn., Nov. 6, 1887.

Harlan, Joseph G., Haverford, Pa. (8).

Harlan, Richard, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Sept. 19, 1796. Died in New Orleans, La., Sept. 30, 1843.

Harris, Geo. H., Rochester, N. Y. (35). Born in West Greece, N. Y., Dec. 29, 1843. Died in Dansville, N. Y., Oct. 5, 1893.

Harris, Thaddeus William, Cambridge, Mass. (1). Born in Dorchester, Mass., Nov. 12, 1795. Died in Cambridge, Mass., Jan. 16, 1856.

Harrison, A. M., Plymouth, Mass. (29).

Harrison, Benjamin Franklin, Wallingford, Conn. (11). Born April 19, 1811. Died April 23, 1886.

Harrison, Jos., jr., Philadelphia, Pa. (12). Born in Philadelphia, Pa., Sept. 20, 1810. Died in Philadelphia, March 27, 1874.

Hart, Simeon, Farmington, Conn. (1). Born Nov. 17, 1795. Died April 20, 1853.

Hartt, Charles Frederick, Ithaca, N. Y. (18). Born in Nova Scotia, Aug. 20, 1840. Died March 18, 1878.

Hastings, Charles W., Kansas City, Mo. (38). Died in Brooklyn, N. Y., Oct. 24, 1892.

Haven, Joseph, Chicago, Ill. (17). Born in Dennis, Mass., Jan. 4, 1816. Died May 23, 1874.

Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.

Hayden, Ferdinand Vandeveer, Philadelphia, Pa. (29). Born in Westfield, Mass., Sept. 7, 1829. Died Dec. 22, 1887.

Hayden, Horace H., Baltimore, Md. (1). Born in Windsor, Conn., Oct. 13, 1769. Died in Baltimore, Md., Jan. 26, 1844.

Hayes, George E., Buffalo, N. Y. (15).

Hayward, James, Boston, Mass. (1). Born in Concord, Mass., June 12, 1786. Died in Boston, Mass., July 27, 1866.

Hazen, William Babcock, Washington, D. C. (30). Born in Hartford, Vt., Sept. 27, 1830. Died Jan. 16, 1887.

Hedrick, Benjamin Sherwood, Washington, D. C. (19). Born in 1826. Died Sept. 2, 1886.

Heighway, A. E., Cincinnati, Ohio (29). Born Dec. 26, 1820. Died Jan. 24, 1888.

Hempstead, G. S. B., Portsmouth, Ohio (29). Born in 1795. Died July 9, 1883.

Hendricks, J. E., Des Moines, Iowa (29). Died June 8, 1893, aged 79.

Henry, Joseph, Washington, D. C. (1). Born in Albany, N. Y., Dec. 17, 1797. Died May 13, 1878.

Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.

Hicks, William C., New York, N. Y. (34). Died in 1885.

Hilgard, Julius Erasmus, Washington, D. C. (4). Born in Zweibrücken, Bavaria, Jan. 7, 1825. Died in Washington, D. C., May 8, 1891.

Hilgard, Theodore Charles, St. Louis, Mo. (17). Born in Zweibrücken, Bavaria, Feb. 28, 1828. Died March 5, 1875.

Hill, Walter N., Chester, Pa. (29). Born Apr. 15, 1846. Died Mar. 29, 1884.

Hincks, William, Toronto, C. W. (11). Born in 1801. Died July, 1871.

Hitchcock, Edward, Amherst, Mass. (1). Born in Deerfield, Mass., May 24, 1793. Died Feb. 27, 1864.

Hoadley, John Chipman, Boston, Mass. (29). Born Dec. 10, 1818. Died Oct. 21, 1886.

Hobbs, A. C., Bridgeport, Conn. (28). Died in Nov., 1891.

Hockley, Thomas, Philadelphia, Pa. (33). Died March 12, 1892.

Hodgson, William Ballantyne, Savannah, Ga. (10). Born in Edinburgh, Scotland, in 1815.

Hogsett, John J., Danville, Ky. (39). Died Jan. 18, 1891.

Holbrook, John Edwards, Charleston, S. C. (1). Born in Beaufort, S. C., Dec. 30, 1796. Died in Norfolk, Mass., Sept. 8, 1871.

Holman, Mrs. S. W., Boston, Mass. (29). Died May 5, 1885.

Holmes, Edward J., Boston, Mass. (29). Died in July, 1884.

Homes, Henry A., Albany, N. Y. (11). Born in Boston, Mass., March 10, 1812. Died in Albany, N. Y., Nov. 3, 1887.

Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.

Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.

Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.

Hopkins, Wm., Lima, N. Y. (5). Died in March, 1867.

Hoppock, Albert Eugene, Hastings-on-Hudson, N. Y. (29).

Horsford, Eben Norton, Cambridge, Mass. (1). Born in Moscow, N. Y., July 27, 1818. Died in Cambridge, Mass., Jan. 1, 1893.

Horton, C. V. R., Chaumont N. Y. (10). Died in 1862.

Horton, William, Craigville, N. Y. (1).

Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.

Hough, Franklin Benjamin, Lowville, N. Y. (4). Born in Martinsburgh, N. Y., July 20, 1822. Died June 11, 1885.

Houghton, Douglas, Detroit, Mich. (1). Born in Troy, N. Y., Sept. 21, 1809. Died Oct. 13, 1845.

Hovey, Edmund O , Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.

Howland, Edward Perry, Washington, D. C. (29). Born in Ledyard, N. Y., July 20, 1825. Died in Harrisburg, Pa., Sept. 12, 1888.

Howland, Theodore, Buffalo, N. Y. (15).

Hoy, Philo Romayne, Racine, Wis. (17). Born in Richland, Ohio, Nov. 3, 1816. Died in Racine, Wis., Dec. 8, 1892.

Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.

Hunt, Edward Bissell, Washington, D. C. (2). Born in Livingston Co., N. Y., June 15, 1822. Died in Brooklyn, N. Y., Oct. 2, 1863.

Hunt, Freeman, New York, N. Y. (11). Born in Quincy, Mass., March 21, 1804. Died in Brooklyn, N. Y., March 2, 1858.

Hunt, Thomas Sterry, New York, N. Y. (1). Born in Norwich, Conn., Sept. 5, 1826. Died in New York, N. Y., Feb. 12, 1892.

Husted, Nathaniel C., Tarrytown-on-Hudson, N. Y. (36). Died Nov. 19, 1891.

Hyatt, Theodore, Chester, Pa. (30).

Ives, Moses B., Providence, R. I. (9). Died in 1857.

Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles Thomas, Boston, Mass. (1). Born in Plymouth, Mass., June 21, 1805. Died Aug. 28, 1880.

James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.

Jeffries, John Amory, Boston, Mass. (38). Born in Milton, Mass., Sept. 2, 1859. Died in Boston, Mass., March 26, 1892.

Johnson, Hosmer A., Chicago, Ill. (17). Died in Chicago, Feb. 26, 1891.

Johnson, Walter Rogers, Washington, D. C. (1). Born in Leominster, Mass., June 21, 1794. Died April 26, 1852.

Johnson, William Schuyler, Washington, D. C. (31). Born Sept. 20, 1859. Died Oct. 6, 1883.

Jones, Catesby A. R., Washington, D. C. (8).

Jones, Henry A., Portland, Me. (29). Died Sept. 3, 1883.

Jones, James H., Boston, Mass. (28).

Joy, Charles Arad, Stockbridge. Mass. (8). Born in Ludlowville, N. Y., Oct. 8, 1823. Died in Stockbridge, Mass., May 29, 1891.

Kedzie, W. K., Oberlin, Ohio (25). Born in Kalamazoo, Mich., July 5, 1851. Died in Lansing, Mich., Apr. 10, 1880.

Keely, George W., Waterville, Me. (1). Died in 1878.

Keep, N. C., Boston, Mass. (13). Died in March, 1875.

Kellogg, James H., Rochester, N. Y. (29). Died Dec. 6, 1891.

Kendall, H. D., Grand Rapids, Mich. (35). Died in Guarymas, Mexico, Jan. 28, 1891.

Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.

Kerr, Washington Caruthers, Raleigh, N. C. (10). Born May 24, 1827. Died Aug. 9, 1885.

Kidder, Henry Purkitt, Boston, Mass. (29). Born Jan. 8, 1823. Died Jan. 28, 1886.

King, Mary B. Allen, Rochester, N. Y. (15). Born in Woodstock, Vt., Jan. 26, 1799. Died in Rochester, April 3, 1893.

King, Mitchell Charleston, S. C. (3). Born in Scotland, June 8, 1783. Died Nov. 12, 1862.

Kirkpatrick. James A., Philadelphia, Pa. (7). Died June 3, 1886.

Kite, Thomas, Cincinnati, Ohio (5). Died Feb. 6, 1884.

Klippart, John H., Columbus, Ohio (17). Died October, 1878.

Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.

Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacey, O. M., Crawfordsville, Ind. (39). Died Jan. 9, 1891.

Lacklan, R., Cincinnati, Ohio (11).

Lamb, Mrs. Martha J., New York, N. Y. (29). Died in Jan., 1893, aged 64.

Lapham, Increase Allen, Milwaukee, Wis. (3). Born in Palmyra, N. Y., March 7, 1811. Died in Oconomowoc, Wis., Sept. 14, 1875.

Larkin, Ethan Pendleton, Alfred Centre, N. Y. (33). Born Sept. 20, 1829. Died Aug. 23, 1887.

LaRoche, Réné, Philadelphia, Pa. (12). Born in Philadelphia, Pa., 1795. Died in Philadelphia, Dec., 1872.

Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died Jan. 31, 1852.

Lawford, Frederick, Montreal, Canada (11). Died in 1866.

Lawrence, Edward, Charlestown, Mass. (18). Born June, 1810. Died Oct. 17, 1885.

Lea, Isaac, Philadelphia, Pa. (1). Born in Wilmington, Del., March 4, 1792. Died Dec. 8, 1886.

LeConte, John Lawrence, Philadelphia, Pa. (1). Born in New York, May 13, 1825. Died Nov. 15, 1883.

Lederer, Baron von, Washington, D. C. (1).

Lee, William, Washington, D. C. (29). Died March 2, 1893.

Leidy, Joseph, Philadelphia, Pa. (7). Born in Philadelphia, Sept. 9, 1823. Died in Philadelphia, April 30, 1891.

Leonard, Rensselaer, Mauch Chunk, Pa. (33). Born in Hancock, N. Y., April 12, 1821. Died in Mauch Chunk, Pa., Oct. 26, 1888.

Lewis, Henry Carvill, Philadelphia, Pa. (26). Born in Philadelphia, Pa., Nov. 16, 1853. Died in Manchester, England, July 21, 1888.

Libbey, Joseph, Georgetown, D. C. (31). Died July 20, 1886.

Lieber, Oscar Montgomery, Columbia, S. C. (8). Born Sept 8, 1830. Died June 27, 1862.

Lilly, William, Mauch Chunk, Pa. (28). Died Dec. 1, 1893, aged 73.

Lincklaen, Ledyard, Cazenovia, N. Y. (1). Born in Cazenovia, N. Y, Oct. 17, 1820. Died April 25, 1864.

Linsley, James Harvey, Stafford, Conn. (1). Born in Northford, Conn., May 5, 1787. Died in Stratford, Conn., Dec. 26, 1843.

Lockwood, Moses B., Providence, R. I. (9). Died in 1872.

Lockwood, Samuel, Freehold, N. J. (18). Born in Mansfield, England. Died Jan. 9. 1894, aged 75.

Logan, William Edmond, Montreal, Canada (1). Born in Montreal, Canada, April 23, 1798. Died in Wales, June 22, 1875.

Loiseau, Emile F., Brussels, Belgium (33). Died April 30, 1886.

Loomis, Elias, New Haven, Conn. (1). Born in Willington, Conn., Aug. 7, 1811. Died in New Haven, Conn., Aug. 15, 1889.

Loosey, Charles F., New York, N. Y. (12).

Lothrop, Joshua R., Buffalo, N. Y. (15).

Lovering, Joseph, Cambridge, Mass. (2). Born in Charlestown, Mass., Dec. 25, 1813. Died in Cambridge, Mass., Jan. 18, 1892.

Lowrie, J. R., Warriorsmark, Pa. (29). Died Dec. 10, 1885.

Lucas, Mrs. John, Philadelphia, Pa. (33). Died May 8, 1893.

Lull, Edward Phelps, Washington, D. C. (28). Born Feb. 20, 1836. Died March 5, 1887.

Lyford, Moses, Springfield, Mass. (22). Born in Mt. Vernon, Me., Jan. 31, 1816. Died in Portland, Me., Aug. 4, 1887.

Lyman, Chester Smith, New Haven, Conn. (4). Born in Manchester, Conn., Jan. 13, 1814. Died in New Haven, Conn., in 1889.

Lyon, Sidney S, Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June 24, 1872.

M'Conihe, Isaac, Troy, N. Y (5).

McCutchen, A. R, Atlanta, Ga. (25). Died Nov. 21, 1887.

McElrath, Thomas, New York, N. Y. (36). Born in Williamsport, Pa., May 1, 1807. Died in New York, N. Y., June 6, 1888.

McFadden, Thomas, Westerville, Ohio (30). Born Nov. 9, 1825. Died Nov. 9, 1883.

McFarland, Walter, New York, N. Y. (36). Died July 22, 1888.

MacGregor, Donald, Houston, Texas (33). Died in Oct., 1887.

McLachlan, J. S., Montreal, Can. (31).

McMahon, Mathew, Albany, N. Y. (11).

McNiel, John A., Binghamton, N. Y. (35). Died in Binghamton, Dec. 20, 1891, aged 75.

Maack, G. A., Cambridge, Mass. (18). Died in Aug., 1873.

Macfarlane, James, Towanda, Pa. (29). Died in 1885.

Mackintosh, James B., New York, N. Y. (27). Died in 1891.

Maffet, Wm. Ross, Wilkes Barre, Pa. (33). Died in June, 1890.

Mahan, Dennis Hart, West Point, N. Y. (9). Born in New York, N. Y., April 2, 1802. Died in New York, Sept. 16, 1871.

Marler, George L., Montreal, Can. (31).

Marsh, Dexter, Greenfield, Mass. (1). Born in Montague, Mass., Aug. 22, 1806. Died in Greenfield, Mass., April 2, 1853.

Marsh, James E., Roxbury, Mass. (10).

Martin, Benjamin Nichols, New York, N. Y. (23). Born in Mount Holly, N. J., Oct. 20, 1816. Died in New York, N. Y., Dec. 26, 1883.

Martindale, Isaac C., Camden, N. J. (26). Died Jan. 3, 1893.

Mather, William Williams, Columbus, Ohio (1). Born in Brooklyn, Conn., May 24, 1804. Died in Columbus, Ohio, Feb. 27, 1859.

Maude, John B., St. Louis, Mo. (27). Died in April, 1879.

Maupin, S., Charlottesville, Va. (10).

May, Abigail Williams, Boston, Mass. (29). Born in Boston, April 21, 1829. Died in Boston, Nov. 30, 1888.

Meade, George Gordon, Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.

Meek, Fielding Bradford, Washington, D. C. (6). Born Dec. 10, 1817. Died Dec. 21, 1876.

Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.

Metcalf, Caleb B., Worcester, Mass. (20). Died July 31, 1891.

Minifie, Wm., Baltimore, Md. (12). Born Aug. 14, 1805. Died Oct. 24, 1880.

Mitchel, Ormsby MacKnight, Cincinnati, Ohio (3). Born in Union Co., Ky., July 28, 1810. Died in Beaufort, S. C., Oct. 30, 1862.

Mitchell, Miss Maria, Lynn, Mass. (4). Born in Nantucket, Mass., Aug. 1, 1818. Died in Lynn, 1889.

Mitchell, William, Poughkeepsie, N. Y. (2). Born in Nantucket, Mass., Dec. 20, 1791. Died in Poughkeepsie, N. Y., April 19, 1868.

Mitchell, Wm. H., Florence, Ala. (17).

Mitivier, M. M., Holyoke, Mass. (40). Died in July, 1892.

Monroe, Nathan, Bradford, Mass. (6). Born in Minot, Me., May 16, 1804. Died in Bradford, Mass., July 8, 1866.

Monroe, William, Concord, Mass. (18). Died April 27, 1877.

Moore, E. C., New York, N. Y. (30).

Morgan, Lewis Henry, Rochester, N. Y. (10). Born near Aurora, N. Y., Nov. 21, 1818. Died Dec. 17, 1881.

Morgan. Mrs. Mary E., Rochester, N. Y. (31). Died in 1884.
Morison, N. H., Baltimore, Md. (17). Born in 1815. Died Nov. 14, 1890.
Morris, John B., Nashville, Tenn. (26).
Morris, Wistar, Philadelphia, Pa. (33). Died March 23, 1891.
Morton, Samuel George, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Jan. 26, 1799. Died in Philadelphia, May 15, 1851.
Mott, Alexander B., New York, N. Y. (36). Died Aug. 12, 1889.
Mudge, Benjamin Franklin, Manhattan, Kansas (25). Born in Orrington, Me,, Aug. 11, 1817. Died Nov. 21, 1879.
Muir, William, Montreal, Can. (31). Died July, 1885.
Mussey. William Heberdom, Cincinnati, Ohio (30). Born Sept. 30, 1818. Died Aug. 1, 1882.

Nagel, Herman, St. Louis, Mo. (30). Born in Tritzwalk, Germany, May 28, 1820. Died in St. Louis, Mo., Feb. 18, 1889.
Nettleton, Charles, New York, N. Y. (30). Born in Washington, Conn., Oct. 2, 1819. Died in New York, N. Y., May 5, 1892.
Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Newton, John, Pensacola, Fla. (7). Born near Pittsburgh, Pa., April 22, 1814. Died in Pensacola, Nov. 25, 1893.
Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.
Nichols, William Ripley, Boston, Mass. (18). Born April 30, 1847. Died July 14, 1886.
Nicholson, Thomas, New Orleans, La. (21).
Nicollet, Jean Nicholas, Washington, D. C. (1). Born in Savoy, France, July 24, 1786. Died in Washington, D. C., Sept. 11, 1843.
Northrop, John I , New York, N. Y. (36). Died June 26, 1891.
Norton, John Pitkin, New Haven, Conn. (1). Born July 19, 1822. Died Sept. 5, 1852.
Norton, Lewis Mills, Boston, Mass. (29). Born in Athol, Mass., Dec. 26, 1855. Died in Auburndale, Mass., April 26, 1893.
Norton, William Augustus, New Haven, Conn. (6). Born in East Bloomfield, N. Y., Oct. 25, 1810. Died Sept. 21, 1883.
Noyes, James Oscar, New Orleans, La. (21). Born in Niles, N. Y., June 14, 1829. Died in New Orleans, La., Sept. 11, 1872.
Nutt, Cyrus, Bloomington, Ind. (20). Born in Trumbull Co., Ohio, Sept. 4, 1814. Died in Bloomington, Aug. 23, 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.
Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.
Ogden, William Butler, High Bridge, N. Y. (17). Born in New York, N. Y., 1805. Died in New York, Aug. 3, 1877.
Oliver, Miss Mary E., Ithaca, N. Y. (20).
Olmsted, Alexander Fisher, New Haven, Conn. (4). Born Dec. 20 182 2 Died May 5, 1853.

Olmsted, Denison, New Haven, Conn. (1). Born in East Hartford, Conn., June 18, 1791. Died in New Haven, Conn., May 13, 1859.

Olmsted, Denison, jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.

Orton, James, Poughkeepsie, N. Y. (18). Born in Seneca Falls, N. Y., April 21, 1830. Died in Peru, S. A., Sept. 24, 1877.

Osbun, Isaac J., Salem, Mass. (29).

Otis, George Alexander, Washington, D. C. (10). Born in Boston, Mass., Nov. 12, 1830. Died Feb. 29, 1881.

Owen, Richard, New Harmony, Ind. (20). Born in Scotland, Jan. 6, 1810. Died in New Harmony, March 24, 1890.

Packer, Harry E., Mauch Chunk, Pa. (30). Died Feb. 1, 1884.

Painter, Jacob, Lima, Pa. (23). Died in 1876.

Painter, Minshall, Lima, Pa. (7).

Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.

Parkman, Samuel, Boston, Mass. (1). Born in 1816. Died Dec. 15, 1854.

Parry, Charles C., Davenport, Iowa (6). Born in Admington, Worcestershire, Eng., Aug. 28, 1823. Died in Davenport, Iowa, Feb. 20, 1890.

Parsons, Henry Betts, New York, N. Y. (30). Born Nov. 20, 1855. Died Aug. 21, 1885.

Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.

Pearson, H. G., New York, N. Y. (36).

Pease, F. S., Buffalo, N. Y. (35). Died Nov. 6, 1890.

Pease, Rufus D., Philadelphia, Pa. (33). Died in 1890.

Pedrick, Mrs. William R., Lawrence, Mass. (33).

Peirce, Benjamin Osgood, Beverly, Mass. (18). Born in Beverly, Sept. 26, 1812. Died in Beverly, Nov. 12, 1883.

Peirce, Benjamin, Cambridge, Mass. (1). Born in Salem, Mass., April 4, 1809. Died in Cambridge, Mass., Oct. 6, 1880.

Perch, Bernard, Frankford, Pa. (35). Born in 1850. Died in 1887.

Perkins, George Roberts, Utica, N. Y. (1). Born in Otsego Co., N. Y., May 3, 1812. Died in New Hartford, N. Y., Aug. 22, 1876.

Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.

Perry, John B., Cambridge, Mass. (16). Born in 1820. Died Oct. 3, 1872.

Perry, Matthew Calbraith, New York, N. Y. (10). Born in South Kingston, R. I., 1795. Died in New York, March 4, 1858.

Phelps, Mrs. Almira Hart Lincoln, Baltimore, Md. (13). Born in Berlin, Conn., July 15, 1793. Died in Berlin, July 15, 1884.

Philbrick, Edw. S., Brookline, Mass. (29). Born in Boston, Mass., Nov. 20, 1827. Died in Brookline, Mass., Feb. 13, 1889.

Phillips, John C., Boston, Mass. (29). Born in 1839. Died Mar. 1, 1885.

Piggot, A. Snowden, Baltimore, Md. (10).

Pim, Bedford Clapperton Trevelyan, London, Eng. (33). Born in England, June 12, 1826. Died Oct., 1886.

Platt, W. G., Philadelphia, Pa. (32). Died Nov., 1885.

Plumb, Ovid, Salisbury, Conn. (9).

Pope, Charles Alexander, St. Louis, Mo. (12). Born in Huntsville, Ala., March 15, 1818. Died in Paris, Mo., July 6, 1870.

Porter, John Addison, New Haven, Conn. (14). Born in Catskill, N. Y., March 15, 1822. Died in New Haven, Conn., Aug. 25, 1866.

Potter, Stephen H., Hamilton, Ohio (30). Born Nov. 10, 1812. Died Dec. 9, 1883.

Pourtalès, Louis François de, Cambridge, Mass. (1). Born March 4, 1824. Died July 19, 1880.

Pruyn, John Van Schaick Lansing, Albany, N. Y. (1). Born in Albany, N. Y., June 22, 1811. Died in Clifton Springs, N. Y., Nov. 21, 1877.

Pugh, Evan, Centre Co., Pa. (14). Born Feb. 29, 1828. Died April 29, 1864.

Pulsifer, Sidney, Philadelphia, Pa. (21). Died March 24, 1884.

Putnam, Mrs Frederick Ward, Cambridge, Mass. (19). Born in Charlestown, Mass., Dec. 29, 1838. Died in Cambridge, Mass., March 10, 1879.

Putnam, J. Duncan, Davenport, Iowa (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.

Redfield, William C., New York, N. Y. (1). Born near Middletown, Conn., March 26, 1789. Died Feb. 12, 1857.

Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.

Richardson, Tobias G., New Orleans, La. (30). Died in New Orleans, May 26, 1892. Aged 65 years.

Robb, James, Fredericton, N. B. (4).

Robinson, Coleman T., Buffalo, N. Y. (15). Born in Putnam Co., N. Y., in 1838. Died near Brewster's Station, N. Y., May 1, 1872.

Rochester, Thomas Fortescue, Buffalo, N. Y. (35). Born Oct. 8, 1823. Died May 24, 1887.

Rockwell, John Arnold, Norwich, Conn. (10). Born in Norwich, Conn., August 27, 1803. Died in Washington, D. C., February 10, 1861.

Roeder, F. A., Cincinnati, Ohio (30).

Rogers, Henry Darwin, Glasgow, Scotland (1). Born in Philadelphia, Pa., Aug. 1, 1808. Died in Glasgow, Scotland, May 29, 1866.

Rogers, James Blythe, Philadelphia, Pa. (1). Born in Philadelphia, Pa., Feb. 11, 1802. Died in Philadelphia, June 15, 1852.

Rogers, Robert Empie, Philadelphia, Pa. (18). Born in Baltimore, Md., March 29, 1813. Died Sept. 6, 1884.

Rogers, William Barton, Boston, Mass. (1). Born in Philadelphia, Pa., Dec. 7, 1804. Died in Boston, May 30, 1882.

Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Rutherford, Lewis M., New York, N. Y. (13). Born in Morrisania, N. Y., Nov. 25, 1816. Died in Tranquility, N. J., May 30, 1892.

Sager, Abram, Ann Arbor, Mich. (6). Born in Bethlehem, N. Y., Dec. 22, 1811. Died in Ann Arbor, Mich., August 6, 1877.

Sanders, Benjamin D., Wellsburg, W. Va (19).
Sawyers, Mrs. Alice M. S., Fort Worth, Texas (34). Died April 25, 1893.
Scammon, Jonathan Young, Chicago, Ill. (17). Born in Whitefield, Me., in 1812. Died in Chicago, Ill., March 17, 1890.
Schæffer, George C , Washington, D. C. (1). Died in 1873.
Schimpff, Robert D., Scranton, Pa. (36).
Schley, William, New York, N. Y. (28). Died in 1882.
Schram, Nicholas Hallock, Newburgh, N. Y. (33). Died in Newburgh, N. Y., aged 54 years, 1 month and 2 days.
Schrenk, Joseph, Hoboken, N. J. (36).
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Seaman, Ezra Champion, Ann Arbor, Mich. (20). Born Oct. 14, 1805. Died July 15, 1880.
Senecal, L. A., Montreal, Can. (31).
Senter, Harvey S., Aledo, Ill. (20). Died in 1875.
Seward, William Henry, Auburn, N. Y. (1). Born in Florida, N. Y., May 16, 1801. Died in Auburn, N. Y., Oct. 10, 1872.
Seymour, William P., Troy, N. Y. (19). Died April 7, 1893.
Sheafer, Peter Wenrich, Pottsville, Pa. (4). Born in Halifax, Pa., March 31, 1819. Died in Brown's Mills in the Pines, N. J., March 26, 1891.
Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.
Sherwin, Thomas, Dedham, Mass. (11). Born in Westmoreland, N. H., March 26, 1799. Died in Dedham, Mass., July 23, 1869.
Sill, Elisha N., Cuyahoga Falls, Ohio (6). Born in 1801. Died April 26, 1888.
Silliman, Benjamin, New Haven, Conn. (1). Born in North Stratford, Conn., August 8, 1779. Died in New Haven, Conn., Nov. 22, 1864.
Silliman, Benjamin, New Haven, Conn. (1). Born in New Haven, Conn., Dec. 4, 1816. Died Jan. 14, 1885.
Simpson, Edward, Washington, D. C. (28). Born in New York, N. Y., March 3, 1824. Died in Washington, D. C., Dec. 1, 1888.
Skinner, George, Kalida, Ohio (33).
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12).
Smith, Charles A., St. Louis, Mo. (27). Died in 1884.
Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec. 26, 1880.
Smith, Mrs. Erminnie Adelle, Jersey City, N. J. (25). Born April 26, 1836. Died June 9, 1886.
Smith, John Lawrence, Louisville, Ky. (1). Born near Charleston, S. C., Dec. 17, 1818. Died Oct. 12, 1883.
Smith, J. V., Cincinnati, Ohio (5).
Smith, James Young, Providence, R. I. (9). Born in Groton, Conn., Sept. 15, 1809. Died March 26, 1876.
Smith, Lyndon Arnold, Newark, N. J. (9). Born in Haverhill, N. H , November 11, 1795, Died in Newark, N. J., December 15, 1865.

Snell, Ebenezer Strong, Amherst, Mass. (2). Born in North Brookfield, Mass., October 7, 1801. Died in Amherst, Mass., Sept., 1877.

Sparks, Jared, Cambridge, Mass. (2). Born in Willington, Conn., May 10, 1819. Died in Cambridge, Mass., March 14, 1866.

Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.

Squier, Ephraim George, New York, N. Y. (18). Born in Bethlehem, N. Y., June 17, 1821. Died in Brooklyn, N. Y., Apr. 17, 1888.

Stearns, Josiah A., Boston, Mass. (29).

Stearns, Silas, Pensacola, Fla. (28). Died Aug. 2, 1888.

Steele, Joel Dorman, Elmira, N. Y. (33). Born in Lima, N. Y., May 14, 1836. Died May 25, 1886.

Steiner, Lewis H., Baltimore, Md. (7). Born in Frederick City, Md., in 1827. Died in Baltimore, April, 1892.

Stevenson, James, Washington, D. C. (29). Born in Maysville, Ky., Dec. 24, 1840. Died in New York, N. Y., July 25, 1888.

Stimpson, Wm., Chicago, Ill. (12). Born Feb. 14, 1832. Died May 26, 1872.

Stone, Leander, Chicago, Ill. (32). Died April 2, 1888.

Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.

St. John, Joseph S., Albany, N. Y. (28). Died Nov. 23, 1882.

Straight, H. H., Chicago, Ill. (25). Died Nov. 17, 1886.

Sturges, George, Chicago, Ill. (37). Born at Putnam, Ohio, May 13, 1838. Died at Lake Geneva, Wis., Aug. 12, 1890.

Sullivan, Algernon Sidney, New York, N. Y. (36). Born April 5, 1826. Died Dec. 4, 1887.

Sullivant, William Starling, Columbus, Ohio (7). Born near Columbus, O., Jan. 15, 1803. Died in Columbus, O., Apr. 30, 1873.

Sutton, George, Aurora, Ind. (20). Died June 13, 1886.

Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born in Stamford, N. Y., Jan. 20, 1778. Died in New York, N. Y., Oct. 3, 1853.

Taylor, Arthur F., Cleveland, Ohio (29). Born Dec. 10, 1853. Died June 28, 1883.

Taylor, Richard Cowling, Philadelphia, Pa. (1). Born in England, Jan. 18, 1789. Died in Philadelphia, Pa., November 26, 1851.

Taylor, Robert N., Tollesboro, Ky. (37). Died Aug. 13, 1888.

Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.

Teschemacher, James Englehert, Boston, Mass. (1). Born in Nottingham, England, June 11, 1790. Died near Boston, Nov. 9, 1853.

Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.

Thompson, Alexander, Aurora, N. Y. (1).

Thompson, Charles Oliver, Terre Haute, Ind. (29). Born in East Windsor Hill, Conn., Sept. 25, 1835. Died in Terre Haute, Ind., March 17, 1885.

Thompson, Harvey M., Oakland, Cal. (17).

Thompson, Zadock, Burlington, Vt. (1). Born in Bridgewater, Vt., May 23, 1796. Died in Burlington, Vt., Jan. 19. 1856.

Thomson, Henry R., Crawfordsville, Ind. (30). Died in 1884.

Thurber, Isaac, Providence, R. I. (9).

Tileman, John Nicholas, Sandy, Utah (33). Born in Horhun, Denmark, March 28, 1845. Died in Salt Lake City, Utah, Sept. 4, 1888.

Tillman, Samuel Dyer, Jersey City, N J. (15). Born April, 1815. Died Sept. 4, 1875.

Tobin, Thomas W., Louisville, Ky. (30). Died Aug. 4, 1883.

Todd, Albert, St. Louis, Mo. (27). Born March 4, 1813. Died April 30, 1885.

Tolderoy, James B., Fredericton, N. B. (11).

Torrey, John, New York, N. Y. (1). Born in New York, N. Y., Aug. 15, 1796. Died in New York, March 10, 1873.

Torrey, Joseph, Burlington, Vt. (2). Born in Rowley, Mass., Feb. 2, 1797. Died in Burlington, Vt., Nov. 26, 1867.

Totten, Joseph Gilbert, Washington, D. C. (1). Born in New Haven, Conn., August 23, 1788. Died in Washington, D. C., April 22, 1864.

Townsend, Howard, Albany, N. Y. (10). Born Nov. 22, 1823. Died Jan. 6, 1867.

Townsend, John Kirk, Philadelphia, Pa. (1). Born Aug. 10, 1809. Died Feb. 16, 1851.

Townsend, Robert, Albany, N. Y. (9). Born 1799. Died Aug. 15, 1866.

Trembley, J. B., Oakland, Cal. (17).

Troost, Gerard, Nashville, Tenn. (1). Born in Bois-le-Duc, Holland, March 15, 1776. Died in Nashville, Tenn., Aug. 14, 1850.

Trowbridge, William Pettit, New Haven, Conn. (10). Born in Troy, Mich., in 1828. Died in New Haven, Aug. 12, 1892.

Tuomey, Michæl, Tuscaloosa, Ala. (1). Born in Ireland, September 29, 1805 Died in Tuscaloosa, Ala., March 20, 1857.

Tupper, Samuel Y., Charleston, S. C. (38). Died in 1891.

Tweedale, John B., St. Thomas, Can. (35). Born in Ormskirk, Lancashire, Eng., Oct. 16, 1821. Died in St. Thomas, Can., Nov. 18, 1889.

Tyler, Edward R., New Haven, Conn. (1). Born Aug. 3, 1800. Died Sept. 28, 1848.

Tyler, Edward R., Washington, D. C. (31). Died in Washington, March 30, 1891.

Vancleve, John W., Dayton, Ohio (1).

Vanuxem, Lardner, Bristol, Pa. (1). Born in Philadelphia, Pa., July 23, 1792. Died in Bristol, Pa., June 25, 1848.

Vasey, George, Washington, D. C. (32). Died in Washington, March 4, 1893.

Vaux, William Sanson, Philadelphia, Pa. (1). Born in Philadelphia, May 19, 1811. Died in Philadelphia, May 5, 1882.

Wadsworth, James Samuel, Genesee, N. Y. (2). Born in Genesee, N. Y., October 30, 1807. Died near Chancellorsville Va , May 8, 1864.

Wagner, Tobias, Philadelphia, Pa. (9).

Walker, J. R., Bay Saint Louis, Miss. (19). Born Aug. 7, 1830. Died June 22, 1887.

Walker, Joseph, Oxford, N. Y. (10).

Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.

Walker, Timothy, Cincinnati, Ohio (4). Born in Wilmington, Mass., Dec. 1, 1802. Died in Cincinnati, Ohio, Jan. 15, 1856.

Walling, H. F., Cambridge, Mass. (16). Died April 8, 1888.

Walsh, Benjamin D., Rock Island, Ill. (17). Born in Frome, England, Sept. 21, 1808. Died in Rock Island, Ill., Nov. 18, 1869.

Walton, Joseph J., Philadelphia, Pa. (29). Born in Barnesville, Ohio, Nov. 1, 1855. Died in Philadelphia, Pa., Oct. 11, 1889.

Walton, Joseph R., Washington, D. C. (40). Died Sept. 23, 1892.

Wanzer, Ira, Brookfield, Conn. (18). Born in New Fairfield, Conn., April 17, 1796. Died in New Milford, Conn., March 5, 1879.

Warnecke, Carl, Montreal, Can. (31). Died May 14, 1886.

Warren, Geo. Washington, Boston, Mass. (18). Died in 1884.

Warren, Gouverneur Kemble, Newport, R. I. (12). Born in Cold Spring, N. Y., Jan. 8, 1830. Died in Newport, R. I., Aug. 8, 1882.

Warren, John Collins, Boston, Mass. (1). Born in Boston, Mass., Aug. 1, 1778. Died in Boston, May 4, 1856.

Warren, Samuel D., Boston, Mass. (29). Born in 1817. Died May 11, 1888.

Waters, Edwin Forbes, Boston, Mass. (29). Born in Petersham, Mass. in July 1822. Died in San Francisco, Cal., April 18, 1894.

Watertown, Charles, Wakefield, Eng. (1). Born in Wakefield, England. Died in Wakefield, May 26, 1865.

Watkins, Samuel, Nashville, Tenn. (26).

Watson, James Craig, Ann Arbor, Mich. (18). Born in Fingal, Canada, Jan. 28, 1838. Died in Madison, Wis., Nov. 23, 1880.

Watson, Sereno, Cambridge, Mass. (22). Died March 9, 1892, in the 66th year of his age.

Webster, Horace B., Albany, N. Y. (1). Born in 1812. Died Dec. 8, 1843.

Webster, J. W., Cambridge, Mass. (1). Born in 1793. Died Aug. 30, 1850.

Webster, M. H., Albany, N. Y. (1).

Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.

Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.

Welsh, John, Philadelphia, Pa. (33). Died May, 1886.

Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.

Wheatland, Henry, Salem, Mass. (1). Born in Salem, Jan. 11, 1812. Died in Salem, Feb. 27, 1893.

Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.

Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.

Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.

Wheildon, Alice W., Concord, Mass. (31). Died June 16, 1893.

Wheildon, William W., Concord, Mass. (13). Born in 1805. Died in Concord, Mass., Jan. 7, 1892.

Whitall, Henry, Camden, N. J. (33).

White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.

Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.

Whitman, Edmund B., Cambridge, Mass. (29). Died Sept. 2, 1883.

Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.

Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.

Whittlesey, Charles, Cleveland, Ohio (1). Born in Southington, Conn., Oct. 5, 1808. Died Oct. 18, 1886.

Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.

Wight, Orlando W., Detroit, Mich. (34).

Wilber, G. M., Pine Plains, N. Y. (19).

Wilder, Graham, Louisville, Ky. (30). Born July 1, 1843. Died Jan. 16. 1885.

Willard, Emma C. Hart, Troy, N. Y. (15). Born in Berlin, Conn., Feb. 23, 1787. Died in Troy, N. Y., April 15, 1870.

Williams, Frank, Buffalo, N. Y. (25). Died Aug. 13, 1884.

Williams, J. Francis, Salem, N. Y. (31). Died in 1891.

Williams, P. O., Watertown, N. Y. (24).

Williamson, Robert S., San Francisco, Cal. (12). Born in New York about 1825.

Wilson, C. H., Belize, British Honduras (30).

Wilson, Daniel, Toronto, Can. (25). Born in Edinburgh, Scotland. Died in Toronto, Aug., 1892.

Wilson, Mrs. Mary V. C., Mobile, Ala. (37). Born in Morengo County, Ala., Jan. 29, 1840. Died near Tullahoma, Tenn., June 24, 1889.

Wilson, W. C., Carlisle, Pa. (12).

Winchell, Alexander, Ann Arbor, Mich. (3). Born in North East, N. Y., Dec. 31, 1824. Died in Ann Arbor, Mich., Feb. 19, 1891.

Winlock, Joseph, Cambridge, Mass. (5). Born in Shelbyville, Ky., Feb. 6, 1826. Died in Cambridge, Mass., June 11, 1875.

Woerd, Chas. Vander, Waltham, Mass. (29). Born in Leyden, Holland, Oct. 6, 1821. Died near Dagget, Cal., Dec. 29, 1888.

Woodbury, Levi, Portsmouth, N. H. (1). Born in Francistown, N. H., Dec. 22, 1789. Died Sept. 4, 1851.

Woodman, John Smith, Hanover, N. H. (11). Born in Durham, N. H., Sept. 6, 1819. Died in Durham, N. H., May 15, 1871.

Woodward, A. E., Jefferson City, Mo. (39). Died in Montana, Sept. 20, 1891.

Woodward, Joseph Janvier, Washington, D. C. (28). Born in Philadelphia, Pa., Oct. 30, 1833. Died near that city, Aug. 17, 1884.

Worthen, Amos Henry, Springfield, Ill. (5). Born Oct. 31, 1813. Died May 6, 1888.

Worthington, George, New York, N. Y. (36). Died Feb. 1, 1892.

Wright, Elizur, Boston, Mass. (31). Born in South Canaan, Conn., Feb. 12, 1804. Died Nov. 20, 1885.

Wright, Harrison, Wilkes Barre, Pa. (29). Born July 15, 1850. Died Feb. 20, 1885.

Wright, John, Troy, N. Y. (1).

Wyman, Jeffries, Cambridge, Mass. (1). Born in Chelmsford, Mass., Aug. 11, 1814. Died in Bethlehem, N. H., Sept. 4, 1874.

Wyckoff, William Cornelius, New York, N. Y. (20). Born in New York, N. Y., May 28, 1832. Died in Brooklyn, N. Y., May 2, 1888.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.

Youmans, Edward Livingston, New York, N. Y. (6). Born in Coeymans, N. Y., June 3, 1821. Died Jan. 18, 1887.

Young, Ira, Hanover, N. H. (1). Born in Lebanon, N. H., May 23, 1801. Died in Hanover, N. H., Sept. 14, 1858.

Zentmayer, Joseph, Philadelphia, Pa. (29). Died, 1887.

# ADDRESS

BY

# PROFESSOR JOSEPH LECONTE

THE RETIRING PRESIDENT OF THE ASSOCIATION.

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## *THEORIES OF THE ORIGIN OF MOUNTAIN RANGES.*

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MOUNTAINS are the focal points of geological interest. In their complex structure are contained all kinds of rocks—sedimentary, eruptive and metamorphic—and in their formation are engaged all geological forces in their greatest intensity. They are the culminating points, the theatres of greatest activity of all geological agencies,—igneous agencies in their formation, aqueous agencies by sedimentation in their preparation and by erosion in their subsequent sculpture. Their discussion therefore is a summation of all the principles of structural and dynamical geology. But they are equally important in historical geology, for the birth of mountains marks the times of great revolutions in the history of the earth and therefore determine the primary divisions of geological time. Evidently therefore the theory of mountains lies at the very basis of theoretic geology and a true theory must throw abundant light on many of the most difficult problems of our science.

But if this is the most important, it is also the most difficult of all geological questions. My object now is to give as briefly as possible the present condition of science on this subject. But in all complex subjects there is a region of comparative certainty and a region of uncertainty—a region of light and a region of twilight. My further object, therefore, will be to separate sharply these two regions from one another, and thus to clear the ground, narrow the field of discussion and direct the course of profitable investigation.

But first of all I must define my subject. A mountain range is a single *mountain individual* born at one time (monogenetic) *i. e.*,

the result of one—though it may be prolonged—earth-effort, as contradistinguished on the one hand from a mountain system which is a family of mountain ranges born at different times (polygenetic) in the same general region, and on the other from ridges and peaks which are subordinate parts—limbs and organs, of such a mountain individual. Now a theory of mountains is essentially a theory of mountain ranges, as thus defined. In all that follows therefore on the subject of mountain structure and origin, we refer to mountain ranges.

## STRUCTURE OF MOUNTAINS.

The origin of mountains must be revealed in their structure. We must therefore give briefly those fundamental points of structure on which every true theory of origin must be founded.

1. *Thickness of mountain sediments.* The enormous thickness of mountain strata is well known, but it is impossible to overstate its fundamental importance. We therefore give some striking examples: The Palæozoic rocks involved in the folded structure of the Appalachian according to Hall are about 40,000 ft. thick. The Palæozoics and Mesozoics in the Wahsatch according to King are about 50,000 ft. thick. The Cretaceous alone, in the Coast Range of California near the Bay of San Francisco, according to Whitney, are 20,000 ft. and in Shasta Co. according to Diller are 30,000 ft. thick. The Mesozoics and Tertiaries of the Alps according to Alpine geologists are 50,000 ft. (Judd, Volcanoes, p. 295). The Upper Palæozoic and Mesozoic of the Uinta according to Powell are 30,000 ft. These are conspicuous examples, but the same is true of all mountains.

It might be objected that these numbers express the general thickness of the stratified crust everywhere—only that in mountains the strata are turned up and their thickness exposed by erosion. But this is not true. For in many cases the strata may be traced away from the mountain, and in such cases they always *thin out* as distance increases. For example, the 40,000 ft. of Appalachian Palæozoics thin out going west until at the Mississippi River they are only 2,000 to 4,000 ft. The Palæozoics, which in the Wahsatch are 30,000 ft., thin out eastward until they are only 1,000 ft. on the Plains. It follows then that *mountains are lines of exceptionally thick sediments.*

2. *Coarseness of mountain sediments.* Mountains are composed mainly of grits, sandstones and shales, *i. e.*, of mechanical sediments —and most conspicuously so along their axial regions. As we go from this region, sometimes in either direction, but especially in one direction, the strata become finer and finer: sandstones giving way to shales and shales to limestones, *i. e.*, mechanical to organic sediments. This is conspicuously true of the Appalachian, in so many ways a typical mountain. As we pass from the eastern ridge westward, grits and sandstones are replaced by shales and these by limestones. Therefore mountains are also *lines of exceptionally coarse sediments.*

3. *Folded structure of mountains.* The folded structure of mountains is perhaps the most universal and certainly the most significant of all their features. But there is great variety in the degree and complexity of the foldings. Sometimes the mountain rises as *one* great fold. The Uinta is an example of this. Sometimes and oftener there are several open folds, like waves of the sea. The Jura is a good example of this. Sometimes and oftenest of all, there are many closely appressed folds. This is the case in the Coast Range of California, in the Appalachian, in the Alps and probably in the Sierra. The Appalachian may be taken again as the type. In this range the folds are most numerous and most closely appressed in the axial region and open out and die away in gentle waves as we go westward. Finally, sometimes in extreme cases, as in the Alps, the Pyrenees and probably the Sierra, the strata of the lateral slopes are thrust in under the central and higher parts, so that the strata of these central parts are overfolded outwards on one or both sides. This is the *Fan-structure* so marked in the Alps and Pyrenees, where the under thrust and overfold are on both sides, but found also in the Appalachian and Sierra, where they are on one side only.

*Amount of folding.* Folded structure implies of course an alternation of anticlines and synclines. The number of these varies with the intensity of the folding. In the Coast Range there are apparently four or five anticlines and corresponding synclines. In the Sierra they cannot be counted but there must be very many so closely appressed that the strata seem to be a continuous series dipping all in the same direction, *i. e.*, steeply toward the axis, for at least thirty miles. They cannot form a single series, for this would make an incredible thickness. It must be a series re-

peated several times by extreme folding; how many it is impossible now to say. In the Appalachian according to Claypole[1] there are about nineteen anticlines and synclines in sixty-five miles and in one part, Cumberland valley, there are eight in sixteen miles. In the Vaudoise Alps, according to Renevier, there are at least seven[2] and in Savoy as many as fifteen.[3] In many cases the foldings are so extreme that the strata first rise as folds, then are pushed over beyond the base as overfolds, and finally broken at the crest and the upper limb pushed over the lower limb many miles horizontally. In the Highlands of Scotland, according to Peach, by overthrust the Archæan is brought over the Silurian and overrides it for ten miles. In the Rocky Mountains of Canada according to McConnell the Cambrian is brought over the Cretaceous and overrides it for seven miles. In the Appalachian of Georgia according to Hayes by overthrust the Cambrian is made to override the Carboniferous for eleven miles.

4. *Cleavage structure.* Closely connected with the last and having a similar significance, viz., lateral squeezing and mashing, is another structure, *Cleavage.* This structure is often associated with folding and both with mountain ranges. It is not so universal as folding only because all kinds of strata are not equally affected by it; being well exhibited only in fine shales. It is important to observe that in slaty cleavage the *strike* of the cleavage planes is the same as that of the strata, and both, the same as the trend of the mountain; and that the *dip* of the cleavage planes is nearly or quite vertical. Whole mountains are thus cleavable from top to bottom.

5. *Granitic or metamorphic axis.* Some mountains are made up wholly of folded strata. This is the case of the Appalachian, the Coast Range and the Jura. But most great mountains consist of a granitic or metamorphic axis with stratified flanks. This is conspicuously the case with the Sierra, the Alps and most other great mountains. So general is this, that the typical structure of ranges may be said to be a granitic axis forming the crest and stratified rocks more or less folded outcropping on the slopes. This very characteristic structure ought to be explained by a true theory of origin.

[1] (Am. Nat., Vol. 19, p. 257, *et seq.*)

[2] Archives des Sciences, Vol. 59, p. 5, 1877.

[3] Archives, Vol. 28, p. 608, 1892; and Vol 25, p. 271, 1893.

6. *Asymmetric form.* Mountains are not usually symmetric, with crest in the middle and slopes equal on the two sides. On the contrary they usually have a long slope on one side and a steeper, often a very abrupt slope, on the other. The crest or axis is not in the middle but nearer to one side. The earth wave seems ready to break and often does break with a great fault on the steeper side. The Uinta is perhaps the simplest example. This range rises as a single great fold, but steeper on the north side where there is a fracture and fault of twenty thousand feet vertical. Of course in this as in all cases the original fault-cliff has crumbled down to a steep slope, or even been destroyed entirely. The Sierra and Wahsatch are remarkable examples of asymmetry. The Sierra rises on the west side from the San Joaquin plains near sea level by a gentle slope fifty to sixty miles long, reaches its crest nearly fifteen thousand feet high and then plunges down by a slope so steep that the desert plain on the east, four thousand to five thousand feet above sea level, is reached in six to ten miles. There is on this side a fault-cliff nearly eleven thousand feet high. The Wahsatch has a similiar form except that the fault-cliff looks westward instead of eastward. It is true that the extreme asymmetry of these two mountains was given them long after their origin and by a different process to be presently described. But even before this last movement they were probably asymmetric, though in a less degree. The Appalachian is perhaps here again a typical mountain. Its long slope is to the west and its crest close to the eastern limit. The Alps, the Apennines, the Carpathians, and the Caucasus, according to Suess, are foreign examples of the same form.

There are many other interesting points of structure that might be mentioned, but they are less significant of mode of origin and therefore omitted in this rapid sketch.

## ANOTHER TYPE OF MOUNTAINS.

I have given the main characteristics of mountains of the usual type, of which the Appalachian, the Coast Range, the Alps and the Pyrenees, may be taken as good examples. But there is another type, different in structure and in mode of origin, to which attention I believe was first called by Gilbert. It is doubtful if this is found anywhere except in the basin and plateau regions and

therefore it may be called the Basin region type. The basin and plateau regions are broken by north and south fissures into great crust-blocks, which by gravitation readjustment have been tilted, *i. e.*, one side heaved up and the other side dropped down, so as to form a series of north and south ridges and valleys. Each ridge rises by a long slope on one side to a crest and then drops by a steep fault-cliff on the other. The ridges, therefore, are extremely asymmetric, but the asymmetry is produced in a different way from that of the usual type. In a word these mountains seem to be the result of a series of enormous parallel normal faults. Such faults are common everywhere, but do not usually give rise to any inequalities which may be dignified by the term mountains; or, if so at one time, have since been levelled by erosion. But those in the Basin region are on so grand a scale and so recent in time, they form very conspicuous orographic features. I have sometimes doubted whether they should be called ranges at all; but when we reflect that at least ten thousand feet of the height of the Sierra is due to normal faulting, it seems impossible to withhold the term. Thus mountains may be divided into two types, viz., mountains formed by folding of strata, and mountains formed by tilting of crust blocks. The structure of the one is anticlinal or diclinal, of the other monoclinal. The Sierra probably belongs to both types. It was formed at the end of the Jurassic as a mountain of the first type, but the whole Sierra block was tilted up on its eastern side without folding, at the end of the Tertiary, and it then became also a mountain of the second type.

A complete theory must explain this type also; but since, from its exceptional character, it must be regarded as of subordinate importance, we shall be compelled to confine our discussion to mountains of the usual type.

### EXPLANATION OF THE PRECEDING PHENOMENA.

In all cases of complex phenomena there have been many theories, becoming successively more and more comprehensive. The citadel of truth is not usually taken at once by storm but only by very gradual approaches. First comes the collection of carefully observed facts. But bare facts are not science. They are only the raw *material* of science. Next comes the grouping of these facts by laws more or less general. This is the beginning of true

science. Every such grouping, or reducing to law, is a scientific explanation and therefore in some sense a theory. At first the grouping includes only a few facts. The explanation or theory lies so close to the facts as to be scarcely distinguishable from them. It is a mere corollary or necessary inference. It is modest, narrow, but also in the same proportion *certain*. Then the group of explained facts becomes wider and wider, the laws more and more general and the theory more daring (but in the same proportion also perhaps more doubtful), until it may at last include the Cosmos itself in its boundless but shadowy embrace.

Now in this gradual approach toward perfect knowledge, there are two very distinct stages. The one consists in explanation of the immediate phenomena in hand. This gives the laws of the phenomena and may be called the *Formal theory*. The other explains the *Cause* of these laws, and may be called the *Causal* or *Physical theory*. All science passes through these two stages. For example, until Kepler, the phenomena of planetary motion were a mere chaotic mass of observed facts without uniting law. Kepler reduced this chaos to order by the discovery of the three great laws which go by his name. This is the *formal* theory of planetary motion. But still there remained the question, Why do planets move according to these beautiful laws? Newton explained this by the law of gravitation. This is the causal or *physical* theory.

But this is so important a distinction, that I must illustrate by examples taken from geological science. All the phenomena of slaty cleavage are completely explained by supposing the whole rocky mass to have been mashed together horizontally and extended vertically. This is the *formal* theory and may be regarded as certain. But still the question remains, How does mashing produce easy splitting in certain directions? The solution of this question is the physical theory and is perhaps a little more doubtful, though I think satisfactorily answered by Tyndall. But still there remains a deeper and more doubtful question. Whence is derived the mashing force? Is it general interior contraction as some think, or is it local expansion as others think? A perfect theory must answer all these questions. Take another example. All the phenomena of the Drift are well explained by the former existence of an ice-sheet moving southward by laws of glacial motion, scoring, polishing and depositing in its course. This is the

formal theory. But still the question remains, What was the cause of the ice-sheet? Was it due to northern elevation, or to aphelion winter concurring with great eccentricity of the earth's orbit? And if due to northern elevation, What was the cause of that elevation? A perfect theory must answer all these questions. Take one more example. All the phenomena of earthquakes are completely explained by the emergence on the surface and a spreading there from a centre of a series of elastic earth-waves. This is the formal theory. It explains the immediate facts observed here on the surface but no more. But still remains the question, What is the *cause*, deep down below, of the concussion which determined the series of earth-waves? This the physical theory is far more doubtful. Or the theory may be made still deeper and wider and proportionately more doubtful. If our theory of the cause of interior concussion be the formation of a fissure or readjustment of a fault as seems in many cases probable there would still remain the question of the cause of great fissures and of their subsequent readjustment by slipping. This is probably as far as geological theory would go; for although cosmogony may go still farther, the interior heat of the earth is usually the final term of strictly geological theories.

I have made this long détour, because I wish to keep clear in the mind these two stages of theorizing in the case of mountain-origin. The formal theory is already well advanced toward a satisfactory condition; the physical theory is still in a very chaotic state. But these two kinds of theories have been often confounded with one another in the popular and even in the scientific mind and the chaotic state of the latter has been carried over and credited to the former also, so that many seem to think that the whole subject of mountain-origin is yet wholly in the air and without any solid foundation.

### I. FORMAL THEORY.

A true formal theory, keeping close to the immediate facts in hand, must pass gradually from necessary inferences from smaller groups to a wider theory which shall explain them all.

*Inferences from* 1 *and* 2, *i. e., thickness and coarseness of sediments.* The thickness of mountain-sediments as we have seen is greatest along the axis and grows less as we pass away from that line. Now where do we find lines of very thick sediments forming

at the present time? The answer is: on sea-bottoms, closely bordering continents. The whole washings of continents accumulate very abundantly along shore-lines and thin out seaward. Mountains were therefore born of sea-margin deposits. This view is entirely confirmed by the character of mountain sediments. We have seen that these are coarsest near the crest, becoming finer and then changing into limestones as we pass farther and farther away from the crest. Now this is exactly what we find in off-shore deposits. They are coarse sands and shingle near shore and then become progressively finer seaward until in open sea, beyond the reach of even the finest mechanical sediments, they are replaced by organic sediments which form limestones. It seems evident therefore that the place of a mountain range before mountain-birth was a marginal sea-bottom receiving abundant sediment from a contiguous continental land-mass. This explains at once the usual position of mountains on the borders of continents, and also where there are several parallel ranges, their successive formation seaward. Here then is one important point gained.

But such enormous thickness as we often find would be impossible unless the conditions of sedimentation on the same spot were continually renewed by *pari passu* subsidence of the sea bottom. And we do indeed find abundant evidence of such *pari passu* subsidence, not only at the present time in places where abundant sediments are depositing, but also in the strata of all mountain ranges. In the forty thousand feet thickness of Appalachian strata nearly every stratum gives evidence by its fossils of shallow water, and often by all kinds of shore marks of *very* shallow water. Therefore the place of mountains while in preparation, in embryo, before birth, was gradually subsiding, as if borne down by the weight of the accumulating sediments, and continued thus to subside until the moment of birth, when of course a contrary movement commenced. The earth's crust on which the sediments accumulated was bent into a great trough, or what Dana calls a geo-syncline. This is another important point gained.

But let us follow out our logic. If the earth's crust yields under increasing weight of accumulating sediments, then ought it also to rise under the decreasing weight of eroded land surfaces. If it sinks by loading it ought also to rise by unloading. And such indeed seems to have been the fact. For if all the strata which have been removed from existing plateaus and mountains were restored,

it would make an incredible height of land. At least ten thousand to twelve thousand feet have been carried away by erosion from the Colorado plateau region and yet eight thousand feet remain. At least thirty thousand feet have been worn away from the Uinta mountains and yet ten thousand feet remain. Evidently there has been a rise *pari passu* with the lightening by erosion.

May we not then safely generalize? May we not conclude with Dutton that the earth in its *general* form and its greater inequalities is in a state of gravitative equilibrium; that the earth is oblate spheroid only because this is the form of gravitative equilibrium of a rotating body,—that ocean basins and continental protuberances exist only because the materials underlying the former are denser and underlying the latter lighter than the average. It is true that the spheroid form of the earth and the sinking and rising of the crust by loading and unloading may be explained on the supposition that the earth is liquid beneath a thin crust, but to this view there are three fatal objections. 1. The cosmic behavior of the earth is that of a rigid solid. This I believe to have been demonstrated. 2. The existence of the present great inequalities of the earth would be impossible, except under the most improbable conditions. For example, if the earth be fluid then the crust must rest as a floating body. But if so, then by the laws of floatation, for every continental protuberance on the upper side there must be a corresponding protuberance in reverse on the other side of the crust, and for every great plateau or mountain range, there must be a corresponding plateau or mountain range in reverse. And taking the difference of specific gravity of the floating crust and the supporting liquid to be as great as that between ice and water, these reverse inequalities must be ten times as great as those at the surface! Can we accept so violent an hypothesis? But (3) repeated experiments, especially very recent ones by Carl Barus,[1] prove that rocks increase very notably in density in the act of solidification, so that a solid crust would undoubtedly break up and sink in a liquid of the same material.

But how then are we to explain gravitative equilibrium in the case of a rigidly solid globe? I answer, by two suppositions. 1. That the earth though rigid as glass or even steel to *rapidly* acting force, yet yields *viscously* to heavy pressure *over large areas* and acting *for long time*. A solid globe of glass six feet in diameter will

[1] Amer. Jour., Vol. 45, p. 1, 1893.

very perceptibly change form under its own weight. How much more the earth under its own gravity. This completely explains the oblateness of the earth even if solid throughout and it had never been liquid at all. The earth though rigid behaves like a very stiffly viscous body; like, for example, the ice of glaciers though very much more stiffly viscous. This viscosity would not at all interfere with its rigidity under the tide-generating influences of the sun and moon, for these are far too rapidly acting.

2. The second supposition necessary is that the earth is *not* absolutely *homogeneous* either in density or in conductivity for heat,—that, in secular cooling and contraction, the denser and more conductive areas cooling and contracting faster went down and became the ocean basins, while the lighter and less conductive areas were left as the more prominent land surfaces. And thus to-day the ocean basins are in gravitative equilibrium with the continental areas, because in proportion as oceanic radii are *shorter*, are the materials also *denser*, and in proportion as the continental radii are *longer*, are the materials also specifically *lighter*. This condition of gravitative equilibrium Dutton calls *Isostasy*.

Thus then the great inequalities of the earth constituting ocean basins and continental surfaces are the result of unequal radial descent of the earth's surface by contraction in its secular cooling. This is by far the most satisfactory theory of these *greatest* inequalities.

In thus following the phenomena of isostasy to their logical conclusion, we seem to have gone beyond the limits of our subject, which is the *theory of mountains;* but the close connection which probably exists between the cause of continents and the cause of mountains justifies the digression if such it may be called.

*Inferences from* 3 *and* 4; *folding and cleavage.* Still adhering closely to observed facts, there are some necessary inferences from folded structure and cleavage. These structures are indisputable proofs that mountain strata have been subjected to enormous *lateral* pressure at right angles to the trend of the axis, by which the whole mass has been mashed together horizontally. But such horizontal mashing must of necessity produce corresponding upswelling along the line of yielding. In a word it is evident that mountains have been uplifted largely at least, if not wholly, by horizontal mashing. The only question that remains is, Is lateral mashing alone sufficient to produce the highest mountains? Let us see.

The amount of uplift in such cases would depend on two things, viz., the thickness of the strata and the amount of mashing. Now as already shown mountain sediments are 30,000, 40,000 and even 50,000 ft. thick. The amount of mashing in many mountains is almost incredible. In the Appalachian it is so extreme that in one place according to Claypole nintey-six miles of original sediments have been crowded into sixteen miles and the shortening of the whole Appalachian breadth is estimated as eighty-eight miles.[1] In the Alps the shortening is estimated by Heim at seventy-two miles or one-half the original breadth of the sediments.[2] In a word we may without exaggeration say that in great mountains the original space is to the folded space as 2 to 1 or even as 3 to 1. Now a crushing of 30,000 ft. of sediments into one-half their original space would double their thickness which is equivalent to a clear elevation of 30,000 ft. But strata are 40,000 and even 50,000 ft. thick. Evidently then this method alone is sufficient to account for the highest mountains in the world even allowing for the enormous erosion which they have suffered.

The same is equally shown by the phenomena of slaty cleavage so often associated with folded structure. Slaty cleavage, as has been demonstrated by experiment, as well as by field observation, is produced by a mashing together of the whole rocky mass in a direction at right angles to the cleavage plane and a corresponding extension in the direction of the *dip* of these planes. Now since the cleavage dip is usually nearly or quite vertical, this means a mashing together *horizontally* and a proportionate extension *vertically*. The amount of mashing together horizontally and extension vertically has been in many cases somewhat accurately estimated. In this case also, as in folding. we have evidence of a mashing of 2 or even 3 into 1 and a corresponding extension vertically of 1 into 2 or even 3. This amount of extension affecting thick strata is sufficient to account for the highest mountains in the world without resorting to any hypothetical force pushing upwards from beneath.

There seems therefore to be no reasonable doubt that *mountains are formed wholly by lateral crushing with proportionate upswelling.* This is a very important point gained. Let us hold it fast. This brings me naturally to the next point.

*Inferences from* 5 *and* 6; *granitic axis and asymmetric form.* A granitic or metamorphic axis is a very general though not a uni-

[1] Am. Nat., Vol. 18, p. 257. [2] Heim, Archives des Sciences, Vol. 64, p. 120, 1878.

versal characteristic of mountains. The old idea (still held by some) was that fused matter was pushed up through and appeared above the parted strata along the crest, as the granite axis, lifting the strata as it were on its shoulders to form the slopes. But it must be observed that the axis is often only metamorphic, not granitic, and moreover that some mountains are composed wholly of folded strata alone. If therefore we regard granite as often only the last term of metamorphism, we may more properly speak of the axes of mountains as metamorphic. If so, then it is not necessary to suppose any vertical uprising of fused matter by volcanic forces at all. On the contrary we would explain the axis thus:

It is evident that accumulating sediments must cause corresponding rise of the interior heat of the earth towards the surface so as to invade the lower parts of the sediments and their included water. Now it is well known from the experiments of Daubrée and others, that in the presence of water even in small quantities rocks become softened and even hydrothermally fused, at the very moderate temperature of 400°–800° F. It is certain then that such thickness of sediments as we know accumulated in preparation for mountain birth must have been softened to a degree proportionate to the thickness, and therefore perhaps semifused or even fused in their lower parts along the line of *thickest* deposit and therefore of greatest subsequent elevation. On cooling after elevation, this submountain fused or semifused matter would form a granitic or metamorphic *core* beneath the highest part. The appearance of this core as an axis along the crest is the result not of upthrust but of *subsequent erosion* greatest along this line.

And this in its turn furnishes a key to the location of mountains along lines of thick sediments. For not only the lower parts of such sediments but also the sea-floor on which they are laid down would be hydrothermally softened or even fused. Thus would be determined a line of *weakness* and therefore a line of yielding to lateral thrust and therefore also a line of crushing, folding and upheaval. The folding and the upswelling and the metamorphism would be greatest along the line of thickest sediments and become less as we pass away from that line. In extreme cases, however, the firmer lateral portions might be jammed in under the softer central portions, on one or both sides, and give rise to the fan-structure characteristic of complexly folded mountains. Or again, in such cases the folds might be pushed clean over and broken at the bend,

and then the upper limb slidden over the lower limb even for miles, forming the wonderful thrust-planes of the Alps, the Appalachian and the Rocky mountains already described. Thus the phenomena under (5) is completely explained.

But mountains are usually asymmetric, the crest being on one side. This is explained as follows: Sedimentary accumulations along shore lines are thickest *near* shore (though not *at* shore) and thin out slowly seaward. The cylinder lens formed by sedimentation is not symmetric, its thickest part being near one side and that the shore side. This thickest line, as we have seen, becomes the crest, which therefore is asymmetrically placed on the land-side or side from which the sediments were derived. The overfolding on the contrary is to the seaward.

## SUMMARY STATEMENT OF THE FORMAL THEORY.

We may therefore group all these inferences and sum up our view of the mode of mountain formation thus:

(1) Mountain ranges, while in preparation for future birth, were marginal sea-bottoms receiving abundant sediment from an adjacent land-mass and slowly subsiding under the increasing weight. (2) They were at first formed and continued for a time to grow, by *lateral pressure* crushing and folding the strata together horizontally and swelling them up vertically along a certain line of easiest yielding. (3) That this line of easiest yielding is determined by the hydrothermal softening of the earth's crust along the line of thickest sedimentation. (4) That this line, by softening, becomes also the line of greatest metamorphism; and by yielding, the line of greatest folding and greatest elevation. But (5) when the softening is very great sometimes the harder lateral strata are jammed in under the crest, giving rise to fan structure, in which case the most complex foldings may be *near* but not *at* the crest. Finally (6) the mountains thus formed will be asymmetric because the sedimentary cylinder-lenses from which they originated were asymmetric.

## SOME EXAMPLES ILLUSTRATING.

It is hardly necessary to enforce these views by illustrative examples. They at once arise in the mind of every geologist. But

there are those in this audience who are not geologists. I therefore select a few examples among our own mountains.

1. *Appalachian.* It is well known that, during the whole Palæozoic, the region now occupied by the Appalachian was the eastern marginal bottom of the great interior Palæozoic sea, receiving abundant sediments from an eastern land mass of Archæan rocks, which then extended far beyond the present limits of the continent and whose western coast-line was a little to the east of the present Appalachian crest. The sediments along this marginal sea-bottom increased in thickness during Cambrian, Silurian, Devonian and Carboniferous (with some changes of physical geography, but without greatly changing the line of sedimentation) until forty thousand feet thickness was reached. Such thickness, of course, could not be attained without *pari passu* subsidence. We have additional evidence of this in shallow-water fossils and even shoremarks at many levels in the series. At the end of the coal-period when forty thousand feet had accumulated, the increasing softening along the line caused it finally to yield to horizontal thrust; the whole mass of strata was crumpled together and swelled up along the line of sedimentation and the Appalachian range was born. The same forces which caused its birth, continued to cause its *growth* for a long time. Subsequent erosion has sculptured it into its present forms, but *has not exposed its granitic core.* The crest is on the east or landward side as we should expect and the overfolds are to the west or toward the sea of that time. This is perhaps the most typical example we have.

2. *Sierra.* If it were not for a subsequent movement, so late as the beginning of the Quaternary, which greatly modified its form, the Sierra too would be a typical range. During the whole Palæozoic and the greater part of the Mesozoic, the place now occupied by the Sierra was the eastern marginal bottom of the Pacific receiving sediments from a continental land-mass in the present Basin region. The shore line changed somewhat at the end of the Palæozoic, but the Sierra region maintained its sea-bottom position. At the end of the Jura when an enormous thickness had accumulated the increasing softening of the crust determined a yielding to lateral thrust and consequent formation of the range. Subsequent erosion has completely removed the strata from the crest and exposed the granitic core as an axis.[1] This axis is here also on the

[1] Sierra granite is not Archæan as has been asserted by some, nor does it all antedate the birth of the range. This is proved (1) by the gradation traceable between slates and granite and (2) by the fact stated by Whitney, by Fairbanks, and by Diller that the granite in many places penetrates the slate as veins.

landward side and the overfolds are to the seaward as in the Appalachian. The erosion of the Cretaceous and Tertiary times probably cut down the Sierra to very moderate proportions and reduced it to an almost senile condition. At the end of the Tertiary, a great fault and bodily uplift of the whole Sierra block on its east side transferred its crest to the extreme eastern margin, greatly increasing its height and rejuvenating its erosive vigor.

3. *Coast range.* The formation of the Sierra transferred the coast line westward of that range and the present place of the Coast Range became marginal sea-bottom, receiving sediment from a now greatly increased land-mass. This continued until the end of the Miocene when the Coast Range was similarly formed. We might multiply examples, but these are deemed sufficient to illustrate the principles.

### MINOR PHENOMENA.

We have given only the most fundamental phenomena, *i. e.*, those which reveal the mode of origin, and upon which therefore a true theory must be founded. But all other minor phenomena associated with mountains are well explained by the view above presented and their explanation confirms the view. For example:

1. *Eruptive phenomena.* We have seen that beneath a mountain, before and at the time of its formation, there is a deep-seated core of liquid or semi-liquid matter. Also, it is evident that the strong foldings of the strata in the act of mountain formation must produce fissures parallel to the folds and to the mountain axis and that these fissures may reach down to the sub mountain liquid matter. In the act of mountain formation therefore the sub-mountain liquid must be squeezed into the fissures forming dikes or through the fissures and poured out on the surface as great lava floods covering sometimes thousands of square miles. In most cases subsequent erosion has swept these overflows clean away leaving only their roots as intersecting dikes. Only the most recent still remain. On these great fissure-eruption lava-fields, ordinary volcanic or crater eruptions continue for ages after the mountain formation ceases. In these, however, materials are ejected not by mountain-making forces, but by the elastic force of vapor from percolating waters. All these eruptive phenomena are therefore associated with mountain ranges.

2. *Faults.* In folding and especially overfolding, the strata are of course often broken and the upper wall of the fissure is pushed

over the lower wall by horizontal thrust often thousands of feet forming reverse faults and so-called thrust-planes. Hence this style of faults is everywhere associated with strongly folded rocks and therefore with mountains and is indisputable evidence of horizontal crushing. In other places than mountains, and in horizontal or gently folded rocks, the other style of faults, *i. e.*, normal faults, are more common.

3. *Mineral veins.* The filling of fissures at the moment of formation with fused matter constitutes dikes; but if not so filled, they are afterwards filled by a slow process of deposit from circulating waters and then they form mineral veins. These, therefore, are also common in mountains.

4. *Earthquakes.* Again, the immense dislocations of strata which we find in faults did not occur all at once, but slowly through great lapse of time, and yet on the other hand not by uniform slipping, but by jerks a little at a time. Every such re-adjustment of the walls of a fissure, whether by increasing lateral pressure (reverse faults), or by gravity (normal faults), gives rise to an earthquake. Earthquakes, therefore, although not confined to, are most common in mountain regions, especially if the mountains are still growing.

Thus, leaving out the monoclinal type which seems to belong to a different category, all the phenomena, major and minor, of structure and of occurrences connected with mountains are well explained by the theory of *lateral pressure* acting on lines of thick sediments accumulated on marginal sea-bottom, and softened by invasion of interior heat. This view is therefore satisfactory as far as it goes, and brings order out of the chaos of mountain phenomena. It has successfully directed geological investigation in the past and will continue to do so in the future.

But there still remains the question, "What *is the cause of the lateral pressure?*" The answer to this question constitutes the *physical theory*.

Thus far I suppose there is little difference of opinion. I have only tried to put in clear, condensed form what most geologists hold. But henceforward there are the most widely diverse views and even the wildest speculations. But let us not imagine, on that account, that we have made no progress in the science of mountain origin. The *formal theory* already given is really, for the geologist, by far the most important part of the theory of mountain origin.

For I insist that for the geologist *formal* theories are usually more important than *physical* theories of geological phenomena. That slaty cleavage is the result of a mashing of strata by a force at right angles to the cleavage planes, is of capital importance to the geologist, for it is a guide to all his investigations. To what property of matter this structure is due, is of less importance to him though of prime importance to the physicist. That the phenomena of the Drift are due to the former existence of a moving ice-sheet is the one thing most important to the geologist, guiding all his investigations. Whether this ice-sheet was caused by geographical or astronomical changes is a question of wider but of less direct interest to him. So in the case of mountain ranges, the most important part of the theory is their origin by *lateral pressure* under the conditions given above. The cause of the lateral pressure, though still of extreme interest, is certainly of less immediate importance in guiding investigations.

### PHYSICAL THEORIES.

The most obvious view of the cause of lateral pressure refers it to the *interior contraction of the earth*. This may be called the

### "CONTRACTIONAL THEORY."

This theory is so well known that I will give it only in very brief outline. It assumes that the earth was once an incandescent liquid and has cooled and solidified to its present condition. At first it cooled most rapidly at the surface and must have fissured by tension. But there would inevitably come a time when the surface being substantially cool, and moreover receiving heat also from the sun, its temperature would be fixed or nearly so, while the incandescent interior would be still cooling and contracting. Such has probably been the case ever since the commencement of the *recorded* history of the earth. The hot interior, now cooling and contracting more rapidly than the cooler crust, the latter following down the ever shrinking nucleus would be thrust upon itself by lateral pressure with a force which is simply irresistible. If the crust were ten times, yea one hundred times, more rigid than it is, it must yield. It does yield along the lines of greatest weakness, *i. e.*, along marginal sea-bottoms as already explained. As a first attempt at a physical theory it seems reasonable and therefore until recently has been generally accepted.

### OBJECTIONS TO THE CONTRACTIONAL THEORY.

It is well known that American geologists have taken a very prominent part in the study of mountain structure and mountain origin. So much so, indeed, that the *lateral pressure theory* in the form given above and interior contraction as its cause, have sometimes been called the "American theory." It is also well known that my name, among others, especially Dana's, has been associated with this view. All I claim is to have put the whole subject, especially the formal theory, in a clearer light and more consistent form.[1] The formal theory I regard as a permanent acquisition. The contractional theory may not be so. It is natural, from my long association with it, that I should be reluctant to give it up, but I am sure that I am willing to do so if a better can be offered. We all dearly love our own intellectual children, especially if born of much labor and thought; but I am sure I am willing like Jephtha of old to sacrifice, if need be, this my fairest daughter on the sacred altar of Truth. Objections have recently come thick and fast from many directions. Some of these I believe can be removed; but others perhaps cannot in the present condition of science and may indeed eventually prove fatal. Time only can show. I state briefly some of these objections.

1. Mathematical physicists assure us that, on any reasonable premises of initial temperature and rate of cooling of the earth, the *amount* of lateral thrust produced by interior contraction would be wholly insufficient to account for the enormous foldings.[2] Let us admit—surely a large admission—that this is so. But this conclusion rests on the supposition that the whole cause of interior contraction is *cooling*. There may be other causes of contraction. If cooling be insufficient, our first duty is to look for other causes. Osmund Fisher has made the suggestion (a suggestion by the way highly commended by Herschel) that the enormous quantity of water-vapor ejected by volcanoes and the probable cause of eruptions is not meteoric in origin as generally supposed, but is original and constituent: water occluded in the interior magma.[3] Tschermak has connected this escape of constituent water from the earth, with the gaseous explosions of the sun.[4] Is it not barely possible that we

[1] "Theory of the formation of the great features of the earth's surface." Am. Jour., Vol. 4, pp. 345 and 460, 1872, and also "Structure and Origin of Mountains, Vol. 16, p. 95, 1878.
[2] Cambridge Phil. Trans., Vol. 12, Part II, Dec., 1878.
[3] Cambridge Phil. Trans., Vol. 12, Part II, Feb., 1875.
[4] Geol. Mag., Vol. 4, p. 569, 1877.

may have in this an additional cause of contraction more powerfully operative in early times but still continuing? See the large quantity of water occluded in fused lavas to be "*spit out*" in the act of solidification. But much still remains in volcanic glass, which by refusion intumesces into lightest froth. Here then, is a second possible cause of contraction. If these two be still insufficient we must look for still other causes before rejecting the theory.

2. Again, Dutton[1] has shown that in a *rigid earth* it is impossible that the effects of interior contraction should be concentrated along certain lines so as to form mountain ranges, because this would require a shearing of the crust on the interior. The yielding according to him would be evenly distributed everywhere and therefore imperceptible anywhere. This is probably true and therefore a valid objection in the case of an earth *equally rigid in every part.* But if there be a subcrust layer of liquid or semi-liquid or viscous or even more movable or more unstable matter, either universal or over large areas, as there are many reasons to think, then the objection falls to the ground. For in that case there would be no reason why the effects of general contraction should not be concentrated on weakest lines as we have supposed.

3. But again, it has been objected that the lines of yielding should not run in definite directions for long distances but irregularly in *all* directions. I believe we may find the answer to this objection in the principle of flow of solids under very slow, heavy pressure. The flow of the solid earth, under pressure in *many* directions might well be conceived as being deflected to the direction of least resistance, *i. e.*, of easiest yielding.

4. But again, it will be objected that the amount of circumferential shortening necessary to produce the foldings of some mountains is simply incredible, for it would disarrange the stability of the rotation of the earth itself. According to Claypole, in the formation of the Appalachian range the circumference of the earth was shortened eighty-eight miles and in the formation of the Alps seventy-two miles. Now this would make a decrease of diameter of the earth of twenty-eight miles in the one case and twenty-three in the other. This would undoubtedly seriously quicken the rotation and shorten the day. This seems indeed startling at first. But when we remember that the tidal drag is all the time retarding the rotation and lengthening the day and much more at one time

[1] Am. Jour., Vol. 7, p. 13, 1874. Penn. Monthly, May, 1876.

than now, we should not shrink from acceptance of a counteracting cause hastening the rotation and shortening the day, and thus giving stability instead of destroying it. We must not imagine that there would be anything catastrophic in this re-adjustment of rotation. Mountains are not formed in a day, nor in a thousand years. It requires hundreds of thousands or even millions of years, if physicists allow us so much.

The objections thus far brought forward, though serious, are by no means unanswerable. But there is one brought forward very recently which we are not yet fully prepared to answer and may possibly prove fatal. I refer of course to the

5. *Level of no strain.* Until recently the interior contraction of the earth was considered roughly and without analysis. It was seen that the surface was already cool and its temperature fixed, while the interior was still hot and cooling; and therefore that the exterior must be thrust upon itself and be crushed. But the phenomena are really far more complex than at first appears. It is necessary to distinguish between two kinds of contraction to which the interior layers are subjected, viz., radial and circumferential. If there were radial contraction only, then undoubtedly every concentric shell as it descended into smaller space would be crushed together laterally. But there is for all layers, except the surface, also a circumferential contraction, and this would have just the opposite effect, *i. e.*, would tend to stretch instead of crush. Therefore wherever the decrease of space by descent is greater than the circumferential contraction, there will be crush, and where the circumferential contraction is greater than the decrease of space by descent, there will be tension and tendency to crack. There would be no real cracking, only because incipient cracks would be mashed out or rather prevented by superincumbent pressure. Where these two are equal to one another there will be no strain of any kind. There is a certain depth at which this is the case. It is called the "level of no strain." To Mellard Reade is due the credit of first calling attention to this very important principle.

Let us analyze the principle more closely. It is admitted that at the surface there is no contraction of any kind. It is also calculated that contractions of all kinds cease at depth of 400 miles. It is believed furthermore that, commencing four hundred miles below the surface and coming upward, the contraction increases very slowly from zero to a maximum at the depth of seventy miles, and

then decreases again more rapidly to zero at the surface. This is shown in diagram, fig. 1. In this figure the curve represents the relative rate of contraction, whether radial or circumferential, of the several layers. We use it, however, only to represent the latter. For, in considering the radial contraction, it is not the relative rate of the several layers that immediately concerns us, but their rate of *radial descent*. Now this is a *summation series* and therefore increases to the very surface, but at different rates of increase. The law of increase of radial descent as we come toward the surface is shown in diagram, fig. 2,[1] in which the rate of increase is greatest at seventy miles, just where the curve changes from concavity to convexity. If now we superpose these two diagrams, fig. 3, the depth at which the two curves, viz., that of circumferential contraction and that of radial descent, intersect, is the level of no strain.

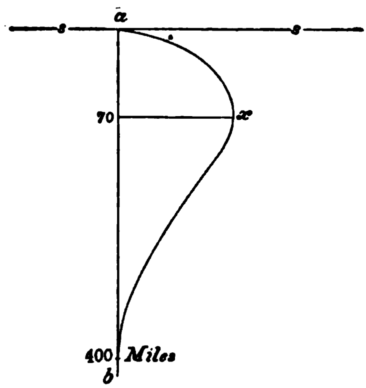


FIG. 1. *s s* = Surface; *a b* = depth along radius; *a x b* = curve of contraction.

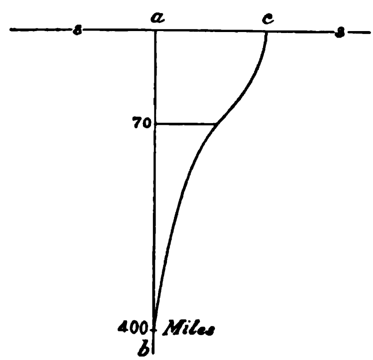


FIG. 2. *c b* = curve of radial descent.

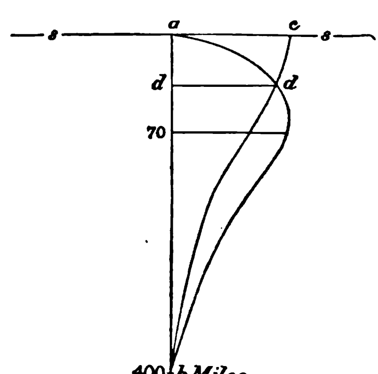


FIG. 3, *d d* = level of no strain.

Now laborious calculations have been made by Davison, Darwin and Fisher to determine the depth of this level of no strain. All make it very superficial. Davison taking an initial temperature of 7000° F. makes it five miles below the surface.

[1] I have taken these figures from Claypole, but modified this one so as to make it a truer representation of the law.

Fisher on the same data only two miles, and with an initial temperature of 4000° only .7 of a mile. It is easy to see that if this be true the amount of lateral thrust must be small indeed.

Now, undoubtedly, there is a true principle here which must not hereafter be neglected, but it is almost needless to say that these quantitative results are in the last degree uncertain. The calculations are of course based on certain premises. These are a uniform initial temperature of say 7000° F., a time of cooling say one hundred or two hundred millions of years, and a certain *rate* of cooling under assumed conditions. The depth of the level of no strain increases with the time and is still going downward. In a word, in a question so complex both mathematically and physically, and in which the data are so very uncertain, every cautious geologist while freely admitting the soundness of the principle will withhold assent to the conclusions. Huxley has reminded us that the mathematical mill, though a very good mill, cannot make wholesome flour without good wheat. It grinds indifferently whatever is fed to it. It has been known to grind peas cods ere now. It may be doing so again in this case. Let us wait.

But besides withholding assent and waiting for more light, I may add that these calculations, of course, go on the supposition that the whole contraction of the earth is due to loss of heat; but as we have already said, it may be due also to loss of constituent water. This would put an entirely different aspect on the subject.

## ALTERNATIVE PHYSICAL THEORIES.

I have given the objections to the contractional theory frankly and I think fairly. They are undoubtedly serious. Let us see what has been offered in its place.

### I. READE'S EXPANSION THEORY.

This, the most prominent among alternative theories, was first brought forward in Mr. Reade's book on "Origin of Mountain Ranges." Although I have carefully read all that Mr. Reade has written on this subject I find it difficult to get a clear idea of his views. But as I understand it, it is in outline as follows: 1. Accumulation of sediments off shore and isostatic subsidence of the same. 2. Rise of isogeotherms and heating of the whole mass of sediments and of the underlying crust in proportion to the thickness

of the sediments. 3. *Expansion* of the whole mass in proportion to rise of temperature. If there were no resistance, this expansion would be in all directions (cubic expansion). 4. But since the containing earth will not yield to expansion laterally, this lateral expansion is satisfied by *folding*, and this in turn produces *vertical upswelling*. Thus the whole *cubic expansion is converted into vertical expansion* which is therefore three times as great as the linear expansion in any one direction. 5. Elevation would of course anyhow be greatest along the line of thickest sediment; but this by itself would not be sufficient to produce a mountain. 6. But farther—and here the theory is more obscure—there is a concentration of the effects of expansion, along a comparatively narrow line of thickest sediments, by a *flow* of the hydrothermally-plastic or even liquid mass beneath, *towards* this central line and then *upward* through the parted strata folding these back on either side and appearing at the crest as the granitic or metamorphic axis. 7. In his latest utterances he seems to adopt the view of Reyer, viz., that the uplifted strata slide back down the slope, producing the enormous crumpling so often found, and exposing a wider area of granite axis. 8. From the same liquid mass which lifts the mountain, come also the great fissure eruptions and the volcanoes.

Mr. Reade makes many experiments to determine the linear expansion of rocks and he thinks that these experiments show that when cubic expansion is converted into vertical expansion and this again concentrated along a line $\frac{1}{4}$ to $\frac{1}{5}$ the whole breadth of the expanding mass, it would explain the elevation of the highest mountains. But still he seems uncertain if it be enough. In fact he declares that if it were not for another factor yet unmentioned, he probably would never have brought forward the theory at all.

9. This factor is *recurrency of the cause and accumulation of the effects*. And here the previous obscurity becomes intensified. I have read and re-read this part without being able wholly to understand him. He seems to think that when expansion had produced elevation, the mountain thus formed would not come down again by cooling and contraction; but on the contrary would *wedge up by normal faulting and set in its* elevated position. Afterward, by new accumulation of heat, another elevation and setting would take place and the mountain grow higher and so on indefinitely or until the store of heat is exhausted. Therefore he characterizes his theory as that of "*alternate expansion and contraction*," or again as that of "*cumulative recurrent expansion*."

Such is a very brief, perhaps imperfect, but I hope fair outline of Reade's theory. It seems to me that there are fatal objections to it. These I now state.

*Objections*. 1. The first is *inadequacy* to account for the enormous foldings of mountains especially when there is no granite axis to fold back the strata. It is true that Mr. Reade makes comparison between his own and the contractional theory in this regard, and seems to show the much greater effectiveness of his own. This may be true if we accept his premises and compare *equal areas* in the two cases. But the contractional theory *draws from the whole circumference* of the earth and *accumulates* the effects on one line, while in Reade's theory the expansion is of course very *local*.

2. But the fatal objection is that brought forward by Davison. It is this. Sedimentation cannot of course increase the sum of heat in the earth. Therefore the increased heat of sediments by rise of isogeotherms *must be taken from somewhere else*. Is it taken from below? Then the radius below must contract as much as the sediments expand, and therefore there will be no elevation. Is it taken from the containing sides? Then the sides must lose as much as the sediments gain and therefore must contract and make room for the lateral expansion, and therefore there would be no folding and no elevation. I do not see any escape from this objection.

Thus it seems that Reade's theory cannot be accepted as a substitute. Is there any other?

### II. DUTTON'S ISOSTATIC THEORY.[1]

Dutton's discussion of isostasy is admirable, but his application of it to the origin of mountains is weak. The outline is as follows:

Suppose a bold coast line, powerful erosion and abundant sedimentation. The coast rises by unloading and the marginal sea-bottom sinks by loading. Now if isostasy is perfect there will be no tendency to mountain formation. But suppose a piling up of sediments but, on account of earth rigidity, without immediate compensatory sinking and a cutting down of coast land without compensatory rising; then *there would be an isostatic slope toward the land*, and the accumulated and softened sediments would *slide landward* crumpling the strata and swelling them up into a mountain range.

[1] Phil. Soc. of Washington. Bull., Vol. 11, pp. 51-64, 1889.

The fatal objection to this view is that complete isostasy is necessary to renew the conditions of continued sedimentation and therefore to make thick sediments; otherwise the sediment quickly rises to sea level and stops the process of sedimentation at that place. But it is precisely a *want* of complete isostasy which is necessary to make an isostatic slope landward. Dutton refers to Herschel as having suggested a similar cause of strata crumpling and slaty cleavage,[1] but the principles involved in the two cases are almost exactly opposite. Herschel supposes sediments to slide down steep *natural* slopes of sea-bottoms, and therefore *seaward*. Dutton supposes sediments to slide *up* natural, though *down* isostatic slopes *landward*. Herschel's is a theory of strata crumpling and slaty cleavage, Dutton's a theory of mountain formation.

There has been no attempt to carry this idea of Dutton's to quantitative detail. It was apparently thrown out as a suggestion in mere despair of any other solution, for he had already repudiated the contractional theory. But the least reflection is sufficient to convince that such slight want of complete isostatic equilibrium as may sometimes occur would be utterly inadequate to produce any such effects.

### III. REYER'S GLIDING THEORY.[2]

Professor Reyer has recently put forward certain views fortified by abundant experiments on plastic materials. His idea in brief seems to be this. Strata are lifted and finally broken through by uprising fused or semifused matters and these appear above as the granitic axis. As the axis rises the strata are carried upward on its shoulders, until when the slope is sufficiently steep the strata slide downward crumpling themselves into complex folds and exposing the granitic axis in width proportioned to the amount of sliding.

No doubt there is much value in these experiments of Reyer, and possibly such sliding does indeed sometimes take place in mountain strata and some foldings may be thus accounted for. But the great objections to this view are (1) that there is no adequate *cause* given for the *granitic uplift*, and (2) that it utterly fails to account for the complex foldings of such mountains as the Appalachian and Coast Range *where there is no granite axis at all.*

[1] Phil. Mag., Vol. 12, p. 197, 1856.
[2] Nature, Vol. 46, p. 224, 1892, and Vol. 47, p. 81, 1892.

Reade indeed holds that the Piedmont region is the granite axis of the Appalachian and that the original strata of the eastern slope are now buried beneath more recent sediment or even beneath the sea. But American geologists are unanimous in the belief that the shore line of the great interior Palæozoic sea was but a little east of the Appalachian crest and the sea washed against land of Archæan rocks extending eastward from that line.

## CONCLUSION.

After this rapid discussion of alternative theories in which we have found them all untenable, we return again to the contractional theory, not indeed with our old confidence, but with the conviction that it is even yet the best working hypothesis we have.

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## *VARIATIONS OF LATITUDE.*

"ALL Astronomy," says Laplace,[1] "depends upon the invariability of the earth's axis of rotation upon the terrestrial spheroid and upon the uniformity of this rotation." He adds that "since the epoch when the application of the telescope to astronomical instruments gave the means of observing terrestrial latitudes with precision no variations in such latitudes have been found which could not be attributed to errors of observation, which proves that since this epoch the axis of rotation has remained very near the same point on the terrestrial surface." Admitting, then, the position of the earth's axis, and consequently the values of terrestrial latitudes, to be sufficiently invariable for the purposes of the astronomer, the question has been many times raised whether this conclusion represents more than a kind of first approximation to the truth.

As this subject, or something very much like it, was receiving more or less attention on the part of the ancient geographers two thousand years ago or more, we can hardly claim for it the charm of novelty. An important feature of the geography of Eratosthenes, written between 200 and 300 B. C., was a critical review of the work of his predecessors. His map of the world, which represented the best and latest information of his day, had, as a sort of base line, or axis of reference, a parallel of latitude drawn from the pillars of Hercules towards the east, passing north of the island of Sicily across the southern part of the Peloponnesus, and eastward across the continent of Asia. The position of many places with

[1]Mécanique Céleste, Tome V, p. 22.

reference to this line differed very considerably from those assigned by his predecessors. At the time of Ptolemy, 400 years later, it was known that the map of Eratosthenes failed in many particulars to conform to the then existing order of things. The conclusion was obvious: evidently changes had taken place in the relative positions of a number of prominent places on the earth, nor were these changes simply the trifling fractions of a second, with which men are struggling so valiantly in these degenerate days—but such satisfactory and tangible quantities as three, four or five degrees. Ptolemy's geography furnished the basis for comparisons and discussions of this kind for fifteen hundred years. Some few of his latitudes, as that of Alexandria, were determined with such precision as was possible in those days, while the foundation of very many was little more than guesswork. Comparisons from time to time with later determination brought to light discrepancies which served to keep the question open and to furnish material for speculation. In this connection we shall stop only to mention Dominique Maria de Ferrare who enjoys the distinction of having had as a disciple the illustrious Copernicus. This philosopher believed that the evidence showed conclusively a progressive change in the position of the pole,—that in time the torrid and frigid regions would in a manner change places.

So far as the latitudes of Ptolemy were concerned it was pointed out[1] that the discrepancies were in part due to the method employed in their determination, that of the gnomon which gave the altitude of the sun's upper limb and consequently a value of the latitude too small by a quarter of a degree.

Two or three hundred years ago much interest was taken in this question. We find associated with it the familiar names of Tycho, Roemer, Hevelius, Picard, Cassini and many others. As greater accuracy in methods and instruments prevailed, it became evident that the rough positions of Ptolemy could not be employed with any confidence in discussions of this character. In connection with these more exact methods also a new phenomenon began to manifest itself, viz., changes of short period.

Christopher Rothman, a contemporary of Tycho, found systematic difference between the determinations of the latitudes of his observatory made in summer and winter. Tycho's observations at

[1] Delambre, Histoire de l'Astronomie au dix-huitième siecle, p. 155.

Prague showed a like peculiarity. Roemer also discovered it. He attributes it confidently to periodic changes in the position of the earth's axis and hopes in time to give a complete theory of the same. A memoir by J. D. Cassini,[1] published in 1693, two hundred years ago almost precisely, gives a very complete summary of the state of the problem at that day. After a detailed examination of the evidence he concludes, "notwithstanding all these apparent variations, we may say that not only has no extraordinary change in the altitude of the pole or in the meridian altitude of the sun occurred in recent times, but that the heavens have at all times occupied the same position with regard to the earth as during the past century. For there is reason to believe that all these variations which have been mentioned come from several defects which occur in observation." He then goes over in detail those sources of error which are so familiar to us—instrumental errors and defects in theory—one only having a somewhat unfamiliar appearance, viz., we may reasonably suppose that variations in the direction of the plumb line occur similar to those of the magnetic needle. Nevertheless, he says it is very probable that from time to time small changes in the altitude of the pole actually do occur, but they are periodic in character and do not exceed two minutes in amount. Thus, instead of several degrees which were conceded by the astronomers of previous centuries, but a paltry two minutes was now allowed; but with improved instruments, with the discovery of aberration and nutation, and the perfection of the theory of refraction, even this modest allowance was gradually reduced to a vanishing quantity.

Meanwhile new arguments were found for a reconsideration of the question. Geology had taken its place among the sciences. In the investigation of the fossil remains of plant and animal life, abundant evidence was found of a former temperate or subtropical climate within the arctic circle. It was also evident that at one time considerable portions of Europe and North America had been covered with glacial ice. Laplace mentions the argument for a change in the position of the earth's axis founded on the existence of the fossil remains of elephants in northern Siberia, but believes that the discovery of the remains of one of these animals

[1] S'il est arrivé du changement dans la hauteur du pole ou dans la cours du soleil? Memoirs de l'Académie, Tome X, p. 360.

preserved in ice, the body of which was covered with thick hair, turns the argument against those who employ it.[1]

In the Quarterly Journal of the Geological Society for 1848 is found a communication from a mathematician and astronomer, Sir John Lubbock—on changes in climate resulting from changes in the earth's axis of rotation. He suggests a mathematical discussion of the problem in order to determine, as he says, "under what hypothesis a change of the position of the axis of rotation is possible or not." The president of the Association, Sir Henry T. de La Beche, in the annual address of 1849, deals at some length with this subject. Again, in 1876, we find Dr. John Evans, then president of the society, discussing the problem.[2]

He describes with much detail the fossil remains found in Spitzenbergen and Greenland belonging to the Miocene, Upper and Lower Cretaceous, Jurassic and other geological periods all of which point to a former temperature much above the present.

Thus, among the Miocene plants of Spitzenbergen, Professor Nordenskiold mentions the swamp cypress, now found in Texas, sequoias of great size, limes, oaks and even magnolias. So in the Lower Cretaceous period Prof. O. Heer distinguished seventy-five species, including ferns, Cycadeæ and Coniferæ, many of which are closely allied to species now found in sub-tropical regions. From these remains, Professor Heer infers that the climate of Greenland and Spitzenbergen during the Cretaceous period was very much the same as that which now prevails in Egypt and the Canary Isles. The existence of beds of coal, of mountain limestone formed of the remains of corals, and bryozoa and shells of marine mollusks; the remains of Ammonites, Nautili, and even a Saurian—the *Ichthyosaurus polaris* — all point in the same direction; while, as Professor Houghton remarks, the arguments from the presence of Ammonites and coal plants strengthen each other, the one demanding heat, the other light. Dr. Evans sums up the argument, as follows: "The three points which it appears to me are most important to bear in mind with regard to the article of flora are: first, that for vegetation, such as has been described, there must according to all analogy have been a greater aggregate amount of summer heat supplied than is now due to such high latitudes; second, that there must have been a far less degree of winter cold

[1] M. C., v, p. 20.

[2] Quarterly Journal Geological Society, 1876, p. 60.

than is in any way compatible with the position on the globe; and, third, that in all probability the amount and distribution of light which at present prevail within the arctic circle are not such as would suffice for the life of the trees."

He afterwards supposes an hypothetical case of possible elevation and depression to which he invites the attention of mathematicians to determine whether it would not produce a change of 15° or 20° in the position of the pole.

The invitation was duly accepted by Sir William Thomson, now Lord Kelvin, and by Mr. G. H. Darwin. The former, by a process which he does not explain, convinced himself that a *vera causa* existed in the distortion of the earth as shown by geological and other evidences sufficient to produce large deviations in the position of the axis. To quote his own eloquent words, "Consider the great facts of the Himalayas and Andes and Africa and the depths of the Atlantic, and America and the depths of the Pacific and Australia; and consider further the ellipticity of the equatorial section of the sea level, estimated by Captain Clarke at about one-tenth of the mean ellipticity of meridional sections of the sea level. We need no brush from the comet's tail to account for a change in the earth's axis; we need no violent convulsions producing a sudden distortion on a great scale, with change of axis of maximum moment of inertia, followed by gigantic deluges, and we may not merely admit, but assert as highly probable, that the axis of maximum inertia and the axis of rotation, always very near one another, may have been in ancient times very far from their present geographical position and may have gradually shifted through ten, twenty, thirty, or forty or more degrees without at any time any perceptible sudden disturbance of either land or water."[1]

Mr. G. H. Darwin has made this the subject of an elaborate mathematical investigation.[2] As the basis, he takes the earth as we find it assuming that the elevations of the continents and depressions of the ocean represent the kind and amount of distortion to which the earth has been subjected in the course of its past history. The mean elevation of the continents being about 1,100 feet and the mean depth of the oceans about 15,000 feet, it follows that in order to convert an ocean bed into a continent or *vice versa*, an elevation or subsidence of 16,000 feet must have taken place. This

[1] British Association Reports, 1876, Sections, p. 11.
[2] Phil. Trans., 1877, p. 271.

would not, however, correctly represent the distortion of the earth, for the water of the ocean flowing into the depressions would considerably modify the result. Taking into account the density of water as compared with the surface rocks, it appears that an extreme elevation of 16,000 feet from the bottom of the ocean to the surface of the supposed continent would be equivalent to an effective elevation of about 10,000 feet on a sealess globe. In case of a perfectly rigid earth the only deformation which could take place would be that due to a re-distribution of the surface materials. For a given elevation with a corresponding depression the maximum effect upon the position of the earth's axis would be produced when the elevations occurred in latitude 45° in two diametrically opposite quarters of the earth with corresponding depressions in the remaining quarters. In such a globe Mr. Darwin's analysis showed that the pole could never have wandered more than 3° from its original position as a consequence of the continents and oceans changing places. If, however, the earth is sufficiently plastic to admit of re-adjustment to new forms of equilibrium by earthquakes or otherwise, possible changes of ten or fifteen degrees may have occurred. This would, however, require such a complete changing about of the continents and oceans, with maximum elevations and depressions in precisely the most favorable places as has certainly never occurred within geologic time. In fact the evidence indicates that the continental areas have always occupied about the same position as now.

It would, therefore, appear that the geologist must give up this hypothesis of great changes in latitude as a factor in the earth's development, unless indeed some other cause can be found of sufficient potency to produce the desired result. Such an agency is perhaps alluded to by Prof. Arthur Shuster in his address before section A of the British Association a year ago.[1] He propounds this question: "Is there sufficient matter in interplanetary space to make it a conductor of electricity?" He adds that he believes the evidence to be in favor of this view; but the conductivity can only be small, for otherwise the earth would gradually set itself to revolve about its magnetic poles. If such an action were admitted, we must suppose the poles of revolution and magnetic poles would long since have been brought into practical coincidence, unless this consummation were frustrated by changes in the position of the latter.

[1] Nature, 1892, Aug. 4, p. 327.

However all this may be, this question before the practical astronomer is this: Have we any reliable evidence showing that progressive changes in the position of the pole are now taking place? If this question were submitted to a jury composed of twelve good men and true from the astronomical profession the chances would be largely in favor of a verdict in agreement with Laplace's decision seventy years ago.

At the international geodetic conference held in Rome in 1883, Mr. Fergola brought forward a plan looking to a systematic study of this and other questions connected with changes of terrestrial latitudes. This plan which was favorably received consisted in a scheme for simultaneous series of observations at pairs of observatories on nearly the same parallel of latitude, but differing widely in longitude. The instruments were to be prime vertical transits and the same stars to be employed at each of the two stations. Several pairs of observatories were designated by Fergola as being favorably situated for the purpose — among others Washington and Lisbon, the difference of latitude being 11′ 7″, that of longitude $4^h\ 31^m$. It is understood that efforts in this direction were made at Washington, but the necessary coöperation at the other end of the line was not secured and the plan came to naught. It has not come to my knowledge that the scheme was at that time seriously considered at any of the other points selected.

Fergola gave a tabular statement which at that time seemed to show small but progressive diminutions of latitude in Europe and North America. This table, with some additions, the latter enclosed in brackets, is as follows:

| | | | |
|---|---|---|---|
| Washington | 1845 | 38° 55′ | 39″.25 |
| | 1863 | | 38.78 |
| | [1883 | | 38.94] |
| Paris | 1825 | 48° 50′ | 13″.0 |
| | 1853 | | 11.2 |
| | [1891 | | 10.95] |
| Milan | 1811 | 45° 27′ | 60″.7 |
| | 1871 | | 59.19 |
| Rome | 1810 | 41° 53′ | 54″.26 |
| | 1866 | | 54.09 |

| | | | |
|---|---|---|---|
| Naples | 1820 | 40° 51′ | 46″.63 |
| | 1871 | | 45.41 |
| Königsberg | 1820 | 54° 42′ | 50″.71 |
| | 1843 | | 50.56 |
| Greenwich | 1838 | 51° 28′ | 38″.43 |
| | 1845 | | 38.17 |
| | 1856 | | 37.92 |

In all these cases there is an apparent diminution during the present century. A similar tendency is shown by the observations of Peters, Gyldèn and Nyrèn at Pulkowa, also by my own observations at Bethlehem, since 1875. Instances are not wanting, however, where this diminution fails to manifest itself. Possibly most of the discrepancies shown here may be referred to periodic changes, the existence of which is no longer in doubt. It is by no means impossible or improbable that small local changes in latitude may occur, due to slipping of the superficial strata of the earth's crust. That such lateral movements have taken place in times past in connection with mountain upheavals is without doubt true. That they are still going on in certain localities is probable; whether they are of sufficient magnitude to admit of measurement can only be determined by observation.

When we remember how few points there are on the surface of the earth whose latitude was determined even no longer ago than fifty years, within one or two seconds of the truth probably, we should suspend judgment for the present with reference to the whole subject of progressive changes.

We come now to a phase of our subject with reference to which we can speak with some confidence, viz., periodic changes.

That, in the case of a perfectly rigid earth, theory points to the existence of such a periodic change completing its cycle in about ten months has been long understood. In connection with the general problem of the motion of a free body under the action of any system of forces, the consideration of which was suggested by the problems of the solar system, we find the names of the leading mathematicians of the last century, d'Alembert, Segner and Euler, not to mention others. It was the latter who, in 1765, in a work entitled "Theory of the Motion of Solid and Rigid Bodies," gave the

equations the final form which Laplace declares seem to him the most simple which can possibly be obtained.[1]

The elegant form of these equations was due to the employment of the principle discovered by Segner, viz., that at every point of a body there are at least three principal axes of inertia at right angles to each other which possess some very important properties. One of these properties is this, that if the body be set revolving about one of these axes, which passes through its centre of inertia and is undisturbed by outside forces, it will continue to revolve about this axis forever. If, however, it be started in its revolution about some other axis, the condition of things will be different.

In the first approximation to the solution of Euler's equations when applied to the earth, we meet with two constants of integration whose values depend upon the position of the axis of revolution with respect to the principal axis of inertia (from which it can never differ very greatly) at the instant which we take at the starting part of our integration.

We further find that the presence of these quantities in our equations shows a revolution of the instantaneous axis of rotation about the principal axis of inertia. This rotation is in the same direction as the diurnal motion, the angular velocity $\eta$ being expressed by the formula $\eta = \frac{C-A}{A} n$, where $n$ is the velocity of the diurnal rotation, $C$ and $A$ the principal moments of inertia of the earth, the first with respect to the polar axis, the second with respect to an equatorial axis, the figure being regarded as that of an ellipsoid of revolution. The ratio $\frac{C-A}{A}$ is found from the value of the constant of nutation, the degree of accuracy being such that the theoretical period of this rotation is known probably within one or two days. The value given by Oppolzer is 304.8 mean solar days.[2] We shall assume it to be 305 days.

The angular distance between the two axes, evidently very small in the case of the earth, can only be determined by observation and will manifest its existence by fluctuations in the latitude having a period of 305 days. The first attempt to find by observation whether or not this movement was appreciable was by Bessel. His method was not well adapted to the purpose and the result was negative or inconclusive.

[1] M. C., v, p. 284.

[2] Lehrbuch zur Bahnbestimmung, Band I, p. 151.

The first quantitative determination which seemed worthy of confidence was made by Dr. C. A. F. Peters of Pulkowa[1] in 1842. From a careful series of meridian circle observations carried on for thirteen months he found for the angle between the two axes $''.079 \pm .017$. Nyrèn followed with a careful discussion of the value given by the observations of Peters, Gyldèn and himself with the same instrument. The results were $''.101$, $''.125$ and $''.058$. Downing found from the Greenwich observations from 1868–77, $''.075$;[2] while Newcomb found the somewhat smaller value $''.04$ from the Washington prime vertical work.

These results are in reasonably good accord and at first sight seemed to show conclusively a real separation of the two axes; but, as pointed out by Hall,[3] the form of the expressions for determining his quantity is such that an apparently real value will always be obtained.

If we assume a uniform rotation of one pole about the other, our equations will contain two unknown quantities $x$ and $y$, where $x = \rho \cos \xi$, $y = \rho \sin \xi$, therefore whatever value we may find for $x$ and $y$, $\rho$ will always have a real and positive value. This may, therefore, be nothing more than a function of the errors of observation.

The true test was, therefore, to be sought in the agreement of the values of $\xi$ when reduced to a common epoch. These were found to be quite discordant, so much so as to throw doubt upon the reality of the results. The truth as we now understand it being that Euler's theory, perfect as it is, does not apply without modification to the present problem, the earth not being strictly a rigid body.

Doubts as to the absolute rigidity of the earth had been expressed by more than one investigator and the matter was discussed in 1876 by Sir William Thomson[4] and in 1879 by Mr. George Darwin[5] in relation to the problems of precession, nutation and tidal action, the conclusion being that the rigidity of the earth is probably between that of steel and glass. The bearing of this upon the present investigation was first pointed out by Newcomb,[6] viz., that in consequence of the elastic yielding of the earth as a

[1] Recherches sur la parallax des etoiles fixes, p. 146.
[2] Monthly Notices R. A. S., Vol. 40, p. 430.
[3] Amer. Journal of Science, March, 1885, p. 223.
[4] British Association Reports, 1876, Sections, p. 11.
[5] Phil. Trans., 1879.
[6] R. A. S. Monthly Notices, March, 1892.

whole the period of this rotation would be lengthened. Before considering this matter in detail, however, the exigencies of historic continuity require us to glance at some remarkable results of observation.

In the spring of 1884, Dr. F. Küstner began a series of observations, the results of which were destined to awaken a widespread interest in this subject, or perhaps more properly, to crystallize the interest which already existed. His original purpose was sufficiently modest. The great meridian circle of the observatory requiring some repairs, he proposed to employ the interval while it was out of service in making a limited series of observations with another instrument, the universal transit, according to the Horribow-Talcott method for the investigation of the constant of aberration. His purpose was not so much that of deriving a new and definitive value of this constant which should be entitled to rank with the excellent results previously obtained, as to test practically the applicability of the method to this purpose and to acquire the experience which at a future time might lead to a favorable result in a more complete series. Possibly it would be overstraining a time-worn simile to compare this modest investigator with Saul, son of Kish, who going forth to seek his father's asses found a kingdom, but certain it is that his results were vastly more important and far-reaching than anything which he could have anticipated in his original program. His observations, not numerous but of the first order of excellence, led to a value of the constant of aberration which appeared to be wholly inadmissible. Many an investigator would have been discouraged with this apparent failure and the world would have known nothing of it.

Not so with Küstner. Instead of abandoning the experiment as a failure he set himself resolutely to work to discover the cause of the anomaly. After examining the various causes which might be supposed to have contributed to such a result, personal, instrumental and refractional, he announced without hesitation that it was due to a change in the latitude itself, viz., that from August to November, 1884, the latitude of Berlin had been from $''.2$ to $''.3$ greater than from March to May in 1884 and '85.

This conclusion was materially strengthened by the examination of a considerable amount of collateral evidence, particularly Nyrèn's elaborate series of observations at Pulkowa from 1879 to 1882, employed by the latter in discussing the constant of aberration.

This somewhat bold hypothesis naturally provoked much discussion and many were sceptical as to its truth; but instead of resorting to polemics and invoking the authority of Aristotle and the sacred scriptures on the one side or the other, means were promptly found for testing it. These comprised both a reëxamination of old observations and new ones undertaken for this express purpose. Among the latter were special series of latitude determinations, extending over an entire year or more, at Berlin, Potsdam, Prague and Bethlehem, all by Talcott's method. All of these agreed most satisfactorily in showing the reality of the fluctuation during the years 1888, '89 and '90.

But the final test which should determine whether the changes observed were due to movements of the earth's axis required observations to be carried on simultaneously, at points differing widely in longitude. A latitude campaign instituted for this purpose was therefore entered upon in the summer of 1891, under the auspices of the International Geodetic Association, operations being carried on at Berlin, Prague, Strasburg, Rockville, San Francisco and Waikiki. Some of the results have been in possession of the public for several months and they show in the most conclusive manner that we are dealing with a movement of the earth's axis.

A series of latitude observations was also carried on at Paris from December, 1890 to August, 1891. Part of the time two different observers were employed using different instruments, their results agreeing almost exactly.[1] Science acknowledges no national allegiance, but it is interesting to note that this series fails to show any trace of the periodic change. Considering the smallness of the quantity in question and the limited scope of the series, this failure proves nothing *pro* or *con*. Yet Admiral Mouchez seizes with alacrity this opportunity for expressing his opinion that the fluctuations, which the Germans have been attributing to changes of latitude, are due to some other cause.[2] It is also noteworthy that the value of the latitude found at this time is ″.8 smaller than that given by the elaborate investigation of M. Galliot in 1879, in which he employed 1077 observations by ten different observers.[3] In this discussion an annual period, having a semi-amplitude of ″.20 manifested itself somewhat obscurely, but M. Galliot places

[1] Comptes Rendus, 1892, p. 895.
[2] Comptes Rendus, 1892, p. 863.
[3] Comptes Rendus, Vol. 111, p. 558.

on record his opinion that this has its origin in some cause other than a change in the latitude.

We have seen how it came about that the reality of periodic fluctuations in the earth's axis was placed beyond dispute. As to the true nature and law of these fluctuations, we should probably now be groping in darkness, but for the services which Dr. S. C. Chandler has rendered in the way of solving the mystery.

Before Mr. Chandler attacked the problem, no one appears to have called in question the applicability of Euler's theory to the case of the earth. The impression was indeed quite general that the changes were for the most part of a fortuitous character, produced by precipitation of rain and snow, by ocean currents and aërial currents acting unequally in different hemispheres; and therefore, in so far as they might manifest a periodicity, this would be annual in its character. As early as 1876, Sir William Thomson expressed the opinion that these causes were sometimes sufficient to produce changes of half a second in the course of a year.[1]

It seemed therefore beyond question that any periodic change must conform to the 305-day period of Euler, or to an annual period, or a combination of the two. The latter hypothesis was worked out very completely by Messrs. R. Radeau[2] and F. R. Helmert.[3]

Matters were in this condition when, in 1891, Chandler attacked the problem. The main features of his investigation are given in a series of seven remarkable papers, published in the Astronomical Journal, written from time to time while the work was still in progress and when, as a matter of course, the final result could not be known. Like Kepler, the author carries us with him along the successive stages of the investigation: we share with him his triumphs and disappointments and rejoice with him when well merited success crowns his efforts.

As to his methods and purpose, these are given in his own words: "I deliberately put aside all teaching of theory, because it seemed to me high time that the facts should be examined by a purely inductive process; that the nugatory results of all attempts to detect the existence of the Eulerian period probably arose from a defect of the theory itself; and that the entangled condition of the whole

[1] British Association Reports, 1876, Sections, p. 11.
[2] Comptes Rendus, Vol. 111. p. 558.
[3] Astronomische Nachrichten, Vol. 126, p. 217.

subject required that it should be examined afresh by processes unfettered by any preconceived notions whatever. The problem which I therefore proposed to myself was to see whether it would not be possible to lay the numerous ghosts in the shape of various discordant, residual phenomena pertaining to determinations of aberration, parallaxes, latitudes and the like, which have heretofore flitted elusively about the astronomy of precision during the century; or to reduce them to some tangible form by some simple consistent hypothesis . . . It was thought that if this could be done a study of the nature of the forces as thus indicated, by which the earth's rotation is influenced, might lead to a physical explanation of them."

From May 29, 1884 to June 25, 1885, almost exactly the time covered by the observations of Küstner at Berlin, Chandler was observing at Cambridge with the Almucantar. The resulting values of the latitude showed a progressive change for which there appeared no explanation, unless the change were that of the latitude itself. At that time this seemed too radical an hypothesis, so the results were printed as they appeared, leaving the explanation to the future. The close agreement with Küstner's results, the verification by the subsequent work at Berlin, Pulkowa, Potsdam and Prague seemed to warrant the expenditure of the labor involved in a thorough investigation of the entire subject.

He began with Küstner's work at Berlin the vertical circle observations of Gyldèn and Nyrèn at Pulkowa and the prime vertical observations of $\alpha$ Lyræ at Washington 1862-66. These agreed in showing a period of 427 days. The examinations of observations of circumpolar stars at Melbourne and of Polaris at Leyden partially confirmed the result.

Next came the observations of Bradley at Kew, Wanstead and Greenwich. Here a very puzzling phenomenon appeared, the period being only about one year, with an amplitude of nearly an entire second. In discussing the observations of Brinkley at Dublin made during the early part of the present century, an opportunity occurred to wrestle, and that successfully, with one of the ghosts before referred to, viz., the singular results which Brinkley had obtained for the parallaxes of a number of stars, and which led to an interesting discussion between Pond and himself.

Thus series after series was analyzed with results in the main encouraging, frequently puzzling, sometimes disappointing. The

law, if such existed, did not appear on the surface. The secret could only be discovered by an elaborate analysis of the material. Accordingly forty-five different series, extending from 1837 to '91, comprising more than 33,000 observations, were examined from which an empirical law was deduced as follows:

The velocity of rotation of the pole was a maximum about 1774, the period being about 348 days. Since then the velocity has diminished at an accelerated rate, the period in 1890 being 443 days.

During the last half century the semi-amplitude has remained sensibly constant at ″.22.

Only three of the forty-five series examined, and those among the least precise intrinsically, gave results contradictory of the general law.

The next step in the process was to analyze the observations in a different manner: to discover whether the deviations from the provisional law were real, also in what manner the variations of the period were brought about.

For this purpose the results were tabulated chronologically at 20-day intervals, all reduced to the meridian of Greenwich. As a result, the real nature of the phenomena was most distinctly revealed and was as follows:

The observed value of the latitude is the resultant curve arising from two periodic fluctuations superposed upon each other. The first of these, and in general the more considerable, has a period of about 427 days and a semi-amplitude of about ″.12. The second has an annual period with a range variable between ″.04 and ″.20 during the last half century.

The maximum and minimum of this annual component of the variation occur at the meridian of Greenwich about ten days before the vernal and autumnal equinoxes respectively, and it becomes zero just before the solstices.

As the resultant of these two motions, the variation of the latitude is subject to systematic alterations in a cycle of seven years' duration, resulting from the commensurability of the two terms. Accordingly as they conspire or interfere the total range varies between two-thirds of a second at a maximum to but a few hundredths of a second at a minimum.

Accompanying this paper is a diagram showing the relation between this theory and the observations of the 54 years on which it is based. The agreement at times almost perfect at other times shows deviation, apparently systematic, which are perhaps due to

imperfect knowledge of the constants, or to erratic deviations of meteorologic origin.

Mr. Chandler finds the general outcome full of promise for the astronomy of precision, showing that observations are free from defects of a systematic character to a much greater extent than has heretofore been supposed.

As the results of which we have been speaking were announced from time to time, they did not pass unchallenged. The reality of the 427-day period was very promptly called in question on account of its supposed conflict with dynamic laws. Professor Newcomb, who at first ranked as a skeptic, soon found a plausible explanation by assuming that the earth is not a rigid body as required by Euler's theory.

The question whether the earth as a whole should be regarded as a rigid body has long been more or less an open one. Certainly the waters of the ocean introduce an element of mobility; but the investigations of Lord Kelvin and Mr. Darwin of the bodily tides in a viscous spheroid when applied to the earth, gave very little, if any, evidence of yielding in case of the latter to external forces.

Laplace had discussed with negative result the effect upon the earth's motion of the mobility of the oceans.[1] Euler's equations had been modified by Liouville for the case of a body which is slowly changing its form from internal causes;[2] and these modified forms had been employed by Darwin in the discussion of the influence of geological changes on the earth's axis of rotation.[3] No suspicion, however, seems to have entered the brain of any of these investigators that any modification of Euler's 305-day period would result either from the mobility of the oceans or the elastic yielding of the earth as a whole.

Newcomb shows in a very simple manner how this result might follow,[4] for, in consequence of this elastic yielding, the pole of figure would be brought toward the pole of the instantaneous axis by the centrifugal force.

Let us call the undisturbed position of the pole of figure the fixed pole, the actual position at any instant the movable pole, and the pole of the instantaneous axis the pole of rotation. The movable pole is therefore constantly moving toward the pole of rotation,

[1] M. C., Tome v, p. 76.
[2] Liouville's Journal, 2nd series, Tome III, 1858, p. 1.
[3] Phil. Trans., 1877, p. 271.
[4] Monthly Notices R. A. S., March, 1892, p. 336.

describing a sort of curve of pursuit: the instantaneous velocity of the latter about the former is that of Euler's period, but the effect of motion of this movable pole is to diminish the angular velocity with respect to the fixed pole in the ratio of its distance from the latter to its distance from the pole of rotation.

Lord Kelvin remarks that this supplies a new and independent method of determining the effective rigidity of the earth. As will readily appear, in this distortion work is being done against resistance; and unless the earth be perfectly elastic, which is certainly not true of that part accessible to observation, the two axes would in time be brought into practical coincidence. The tidal action set up in the oceans would also tend to produce the same result. Apparently, then, the continued existence of this term requires a constantly recurring series of impulses.

Gyldèn remarks that the hypothesis of elasticity is not the only one which will explain the Chandlerian period.[1] He also concludes as the result of a mathematical analysis that we must look for the impelling cause to concussions going on in the interior cavities of the globe.

Aside from the fact that these concussions are in need of explanation to an extent quite equal with that of the phenomenon itself, it is an open question whether any explanation is called for. We have no proof of the perpetuity of this term. We are in possession of no observations accurate enough to throw any light on this subject before the time of Bradley. Nor can it be asserted that so small a coëfficient has remained constant during the interval of one hundred and fifty years: possibly it may be on the road to extinction.

As to the annual term it seems to have no foundation in theory except as the result of meteorological causes, in which case we can hardly hope for more success in dealing with it than in predicting the weather upon which it depends.

For further improvement in our knowledge on this subject, we must look to continued observation at a number of points carried on for this express purpose and so conducted as to eliminate if possible all systematic errors. If, as seems probable, the coëfficients, at least that of the annual term, partake of the erratic nature of meteorological phenomena, it will be necessary to keep up this work perpetually.

[1] Astronomische Nachrichten, Band 132, p. 198.

A plan is under discussion for establishing four permanent latitude stations on the same parallel of latitude at intervals of .90° in longitude as nearly as may be. These will presumably be equipped with identical instruments of the most approved form and the same stars employed at all of them. Until this plan or some modification of it is in working order, and probably for some time after, careful determinations at other points will continue to furnish valuable data especially in settling the question of progressive changes, local or otherwise.

The instrument hitherto employed in special observations for this purpose is the zenith telescope. The possibility of determining latitude by measurement of the small difference of zenith distance of two stars properly situated, one culminating north, the other south of the zenith, was pointed out by Horrebow in his *Atrium Astronomiæ* in 1732.[1] Possibly he may have made a practical application of the principle; if so, any account of it has escaped my notice. The method was, however, employed by Father Hell, otherwise not unknown to fame, in determining the latitude of his transit of Venus station at Wardoehuus in 1769.

He appears to have been unacquainted with Horrebow's previous suggestion and determines his latitude in this way, as he says, from necessity.

The idea seems to have lain dormant until about 1834, when it was hit upon independently by Talcott in this country and Pond in England. The latter, in employing the zenith telescope which had then been recently mounted atthe Royal Observatory for the special purpose of observing $\gamma$ Draconis, found that a fifth-magnitude star passed the meridian thirty minutes later at nearly the same distance on the opposite side of the zenith. By observing these two stars, reversing the instrument between them, he found certain advantages now well known to be inherent in the method.[2] Pond states that the same method may be employed with altazimuths and other portable instruments, but the communication seems to have attracted no attention and apparently he made no attempt to develop it farther.

In striking contrast, is the immediate success which attended the employment by Talcott of an instrument constructed to carry out this principle. His first practical application of it was in 1834 in

[1] Wolf, Geschichte der Astronomie, p. 608.
[2] Phil. Trans., Vol. 124, p. 209.

the survey of the northern boundary of Ohio.[1] Its merits were very promptly recognized by the officers of the U. S. Coast Survey where it received a number of modifications and improvements suggested by experience, making it practically the instrument which we have to-day. It was many years, however, before it came into use to any considerable extent on the eastern side of the Atlantic.

To America undoubtedly belongs the honor of practically introducing this important improvement in latitude determination.

But although Americans practically introduced the instrument to the world, it was reserved to the Germans to teach us how to use it. It is due in great measure to refinements and improvements devised by German observers and instrument makers that the probable error of a single determination is now $''.12$ or $''.15$, instead of three times these amounts with which we were formerly satisfied.

The essential features of this instrument are the micrometer and the level. Unless these are of a high degree of excellence, first-class results cannot be obtained; especially is this true of the level, of which two are commonly employed with the best class of instruments. Only those who have experienced it are aware how difficult it is to procure levels of the necessary quality. Moreover, changes of form are liable to occur, rendering what was a good level worthless. The method so frequently employed of determining the value once for all and continuing to use it for years without further examination will not answer here.

This uncertainty of the level has led to devices for dispensing with it. One of these which seems promising is the floating zenith telescope invented by Fathers Hagen and Fargis. In this instrument the telescope, with its accessories, floats on the surface of a trough of mercury, the trail of the star as it crosses the field being recorded on a photographic plate which may be measured at leisure. Possibly a way may be found for making these exposures automatically, thus furnishing means for keeping a record, continuous in so far as absence of daylight and of clouds will permit.

With four stations established as described above, equipped with automatic instruments, data will be rapidly accumulated for settling the questions still remaining doubtful. It will not, however, be work of merely one, two or three, but many years. Is it too much to hope that within five or ten years we may see some such system as this in full and successful operation?

[1] Journal Franklin Institute, Oct., 1838.

# PAPERS READ.

A CONSTRUCTION FOR THE IMAGINARY POINTS AND BRANCHES OF ALGEBRAIC LOCI. By Prof. F. H. LOUD, Colorado Springs, Col.

[ABSTRACT.]

I. A CONSTRUCTION for imaginaries, to be of service in connection with analytical geometry, should possess the following characters:—

1. *Generality*, embracing all possible combinations of complex coëfficients and coördinates, in equations of all degrees between two variables.
2. *Reducibility* to the usual Cartesian construction, both as regards the geometric forms and the operations, by the mere vanishing of the imaginary terms.
3. *Simplicity*, as great as is compatible with the increase in the number of degrees of freedom given a moving point by the admission of imaginary terms.
4. *Extensibility* to the geometry of surfaces (three variables).

II. Proposed method of construction.[1]

Assuming in a plane an axis of reals and an axis of imaginaries (Argand), let the point (X, Y) be defined as the extremity of the vector, or *stroke* (Harkness and Morley), $\text{X} + i\,\text{Y}$ (X and Y generally complex). Then to each pair of values of X and Y corresponds *one* point in the plane, but each point has a doubly infinite number of sets of coördinates.

An equation, $f_n\,(\text{X}, \text{Y}) = 0$, will have the effect

1. To reduce the *number* of sets of coördinates of each point to the finite value $n$. But when the locus of the equation contains as a $p$-fold point one of the circular points at infinity, this number is further reduced to $n - p$.
2. To determine the *distribution* of these sets of coördinates (or points of the locus) over the plane. Those points for which the coëfficient, $\xi$, of $i$ in the value of X has any fixed value, $\mu$, lie on a line or curve for which the name, the *x-comitant* $\mu$ is proposed, and $\mu$ is called the *characteristic* of the comitant. By assigning successively different values as characteristics, a series of $x$-comitants is obtained; and there is in like manner a series of $y$-comitants.

[1] An outline of the theoretical relation of this to other methods of geometric interpretation of imaginaries may be found in Vol. 8, *Annals of Mathematics;* and an exposition of its application to the equation $\text{X}^2 + \text{Y}^2 = r^2$ in the fourth annual volume of *Colorado College Studies.*

*Note on notation.* Any (constant or variable) quantity regarded as generally complex may be denoted by a small capital letter as M, while the point M in a diagram is the termination of a vector equal to M, extending from 0. When real and imaginary terms are to be separated, M is replaced by the binomial value $m + i\mu$; and if the condition is made that $\mu$ vanishes, m (Roman) is written for $m$.

III. General properties of comitants.

If a locus has a real branch, the latter is a common arc of the $x$-comitant 0 and the $y$-comitant 0.

A point of intersection (or of contact) of two loci is a point at which a comitant of each series belonging to one locus is met (or touched) by the comitant of equal characteristic belonging to the corresponding series of the other locus.

If the point of contact of two loci is at infinity, every comitant of one locus has, as an asymptote, the comitant of like series and equal characteristic of the other locus. (A special case arises in the instance of one of the circular asymptotes.)

Comitants may be constructed, by points, directly from the equation of the locus, if its coëfficients are known numerically. If the latter are given geometrically, as vectors in a diagram, equations of first and second degrees yield by inspection elementary processes of construction, wherein the usual principles of addition and proportion of "strokes" must be observed.

The comitants of a locus of order $n$ are curves of order $n^2$ at most, and their equations in the ordinary Cartesian sense (called *comitant-equations*) may be derived as follows: Substitute their binomial values, as $y + i\eta$ for Y, for all letters of the equation, expand, and equate to zero the sums of the real and of the imaginary terms. Put $\mathrm{x} + \eta$ for $x$, and $\mathrm{y} - \xi$ for $y$. From the resulting equations eliminate either $\eta$, producing the equation of the $x$-comitants, or $\xi$, producing that of the $y$-comitants.

IV. The locus of first order, equation $\mathrm{AX} + \mathrm{BY} + \mathrm{C} = 0$.

All the comitants of a single series are parallel right lines, and, for equal increments of the characteristic, are equidistant. The angle made by the members of one series with those of the other equals that between the vectors A and B, and hence when the ratio of the coëfficients $-\mathrm{B} : \mathrm{A}$ is real, the two series are parallel. Otherwise the two comitants 0 intersect in the *real point* of the locus.

The coëfficients being given geometrically, simple linear constructions determine (1) the real point (which is the intersection of the lines whose Cartesian equations are $ax + by + c = 0$, and $\alpha x + \beta y + \gamma = 0$); (2) the point for which $\mathrm{X} = 0$ and that for which $\mathrm{Y} = 0$ (these with the foregoing determine the comitants zero); (3) the coördinates of the origin regarded as a point of the locus (these with the foregoing indicate the position of every other comitant). When, instead of the coëfficients of the equation, the coördinates of two points of the locus are given, some simplification ensues.

To determine the point of intersection of two linear loci, note that the points in which the $x$-comitants of one are met by the $x$-comitants of equal characteristic in the other lie on a line, and the same is true of the $y$-comitants; hence the intersection of these two lines is the point required.

The comitant equations of the general linear locus are:

$$(ab + a\beta)\,\mathrm{x} + (b^2 + \beta^2 - a\beta + ba)\,\mathrm{y} - [(a - \beta)^2 + (b + a)^2]\,\xi + (bc + \beta\gamma - a\gamma + ca) = 0.$$

$$(a^2 + a^2 - a\beta + ba)\,\mathrm{x} + (ab + a\beta)\,\mathrm{y} + [(a - \beta)^2 + (b + a)^2]\,\eta + (ac + a\gamma + b\gamma - c\beta) = 0.$$

V. Extension to three variables.

A point X, Y, Z is located by constructing on an horizontal plane the vector X + *i* Y and on a vertical plane the vector X + *i* Z, and completing the parallelogram. The comitants of plane curves are replaced by comitant surfaces, of which, for a constant value of $\xi$, there will be two series, one of $x$- $y$-comitants and one of $x$- $z$-comitants.

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## The screw as a unit in a Grassmannian system of the sixth order.[1]

By Prof. E. W. Hyde, Cincinnati, Ohio.

[ABSTRACT.]

The screw, as defined by the author in a paper read before the A. A. A. S. at the Cleveland meeting, viz., the sum of two right lines, one of which is definite and the other is + to it at ∞, *i. e.*, equivalent to a plane vector, is taken as the fundamental unit of a Grassmannian system of screw space of the sixth order. An interpretation is then given to the product of two, three, four, five and six of these units, or *monoids*, as they are designated. The conditions of equality of two products of the same order are determined. The *complement*, as used by Grassmann, is introduced, and progressive and regressive products are treated, the system being as fully developed as is consistent with reasonable brevity.

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## On the possibility of the algebraic solution of the general equation of the fifth degree.

By Prof. Mansfield Merriman, South Bethlehem, Pa.

[ABSTRACT.]

Reference is made to some of the so-called demonstrations of the impossibility of the algebraic solution of the quintic equation, and their unsatisfactory character is pointed out. The present state of the solution of the quintic by means of a resolvent sextic is indicated. The suggestion is made that the sextic is perhaps one whose roots are connected by a relation which will render it reducible to a cubic and a quadratic.

[1] Published in full in Annals of Mathematics, Vol. 8.

APPLICATION OF THE GENERALIZED THEOREM. By Prof. ALEX MACFARLANE, Austin, Texas.

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ON THE INSCRIPTION OF REGULAR POLYGONS. By L. E. DICKSON, Austin, Texas.

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SPACE. By Dr. S. S. LAWS, Columbia, S. C.

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UPON THE LATITUDE VARIATION TIDE. By ALEX. S. CHRISTIE, U. S. Coast Survey, Washington, D. C.

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LATITUDE DETERMINATION AT BETHLEHEM, 1892-3. By Prof. C. L. DOOLITTLE, Bethlehem, Penn.

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A DETERMINATION OF THE CONSTANT OF ABERRATION BY A MODIFIED FORM OF THE LOEWY METHOD. By Prof. GEORGE C. COMSTOCK, Madison, Wis.

SECTION B.

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ADDRESS

OF

# EDWARD L. NICHOLS,

VICE PRESIDENT, SECTION B.

## *PHENOMENA OF THE TIME-INFINITESIMAL.*

SCIENCE consists in the extension of our knowledge of the external universe, and it brings about this extension in great part by reinforcement of our senses. To bring into the field of observation the very distant and the very small are, therefore, regarded as important scientific achievements, the telescope and the microscope, by means of which this widening of the realm of knowledge has been made, as important implements of research.

Man's relation to time is such that it is difficult to conceive of an instrument which could bring distant events to hand in like manner for inspection. Our time vision turns chiefly in one direction,—towards the past,—and is obscured by the intervention of something very like a medium or atmosphere, through which we see dimly. As to the future, our thoughts are necessarily confined to matters found by experience of the past to be periodic, or to changes already begun and known by the observation of analogous processes to be likely to run some definite course. In the interpretation of the future by the past, there is much of interest to the physicist; but it is not of this that I would speak to-day. Let us turn our attention rather to the study of minute time intervals in physics,—to a consideration of the methods by which we may record what takes place during infinitesimal elements of time. The interest of the physicist in time is confined really to a study of phenomena. He ascribes no property to time itself, beyond defining it after Riemann, as a complexity of the first order.[1]

[1] "Eine einfach ausgedehnte Mannigfalltigkeit" (Riemann; Ueber die Hypothesen welche der Geometrie zu grunde liegen. Werke, p. 257).

As between the study of the infinitesimally great and the infinitesimally small, whether of space or of time, there is a peculiar value to be attached to the latter, because the only methods which have proven the least fruitful in the analysis of the more complex changes which are going on around us, are those which begin with the infinitesimal. We consider an element of mass or of volume, or sometimes merely the element of a surface or line, proceeding then to extend our statements in so far as our powers of mathematical expression will permit.

Now the element of time is of course purely relative. In certain phenomena the time infinitesimal is so short as compared with any time interval with which we are able to cope experimentally as to be out of reach, just as in spacial relations the dimensions of the molecule and atom are such that we dare not hope to render these ultimate particles of matter visible even under the microscope. There are periodic phenomena, on the other hand, the periods of which are so great that a life-time, indeed the entire era covered by history and tradition, affords us a glimpse of but a single time element. Lying between these two there is a great range of phenomena for which the element of time is within our reach. It is by the study of what takes place in such time elements and the extension of the results thus obtained by analytical processes that much of our knowledge of physics has been gained, and it is to the extension of our powers in the observation of the phenomena of the time interval that we must look in great part for further progress. It has seemed worth while, therefore, to bring together for purposes of comparison some of the methods which have proven fruitful in this respect, and to consider along what line they may be further developed. It is an investigation which will lead us into all departments of the science; for phenomena into which the element of time does not enter are unknown.

Since all study of phenomena involves the time element, the consideration of all dynamical problems must begin with the *phenomena* of the *time-infinitesimal*. There are two cases of chief importance:

1. The study of the time elements of periodic phenomena.
2. The study of beginnings of changes which result from a sudden variation in the condition of equilibrium.

The methods which have been found most useful in the investigation of the phenomena under consideration may be classified as follows:

1. Visual methods: (a) vision by instantaneous exposure; (b) vision by periodically interrupted exposure; (c) vision by the aid of the revolving mirror.

2. Photographic methods: (a) instantaneous exposure of a stationary film; (b) photography by the aid of the revolving mirror; (c) continuous exposure of a moving plate; (d) successive short time exposures of a moving plate.

3. Indirect graphical and electrical methods.

Much of the most important work which has been done in the domain of sound falls within the scope of our present inquiry, and it is in that field that many of the methods just indicated have been developed. The revolving mirror, for example, is a favorite tool of the acoustician. Its usefulness is too well known to need mention here, but I wish to remind you that this instrument, chiefly used for the separation of images representing phenomena covering intervals of thousandths of seconds, has been found capable of rendering much briefer events subject to inspection and analysis.

The inventor of the revolving mirror (Wheatstone) found it possible to study time intervals down to within a millionth of a second.[1] He obtained a rate of revolution, never since greatly exceeded, I think, of eight hundred revolutions a second. It is evident that he stood at the very threshold of the discovery of the oscillatory discharge, and that it was merely an accident of the relation of resistance and capacity in the circuits which he employed which prevented him from observing that important form of the electric spark. That he was fully aware of the wide range of investigations to which the revolving mirror is adapted, is also clear. He says in the memoir which Faraday presented for him before the Royal Society in 1834: *But this instrument is not confined to observing merely the intermittedness of electric light; whenever a rapid succession of alternations occurs in an object which does not change its place they may be separately examined by this means. Vibrating bodies afford many instances for investigation; one among these is perhaps worthy to be mentioned. A flame of hydrogen gas, burning in the open air, presents a continuous circle in the mirror; but while producing a sound within a glass tube regular intermissions of intensity are observed, which presents a chain-like appearance and indicate alternate contractions and dilations of the flame corresponding with the sonorous vibrations of the column of air.*[2]

[1] Wheatstone; Philosophical Transactions, 1834.

[2] *l. c.*, p. 586.

In a later paragraph of the same paper he noted the applicability of the spark in the study of the phenomena of the time infinitesimal, suggesting a method, the importance of which is even now but imperfectly appreciated. *The instantaneousness of the light of electricity of high tension . . . affords the means of observing rapidly changing phenomena during a single instant of their continued action.*

In the hands of Foucault,[1] Michelson[2] and of Newcomb,[3] the revolving mirror has given us our best determinations of the velocity of light; in those of Fedderson,[4] Rood,[5] Trowbridge,[6] Boys[7] and others, it has made it possible to resolve the oscillatory spark into its elements.

Fedderson's experiments are especially noteworthy, because he succeeded (in 1862)[8] in photographing the discharge of the Leyden jar, securing an excellent record of the images seen in the revolving mirror. We are apt at the present day to look back to the introduction of the dry plate as the step necessary to the application of photography to the study of fleeting phenomena, but certainly the results obtained by this early investigator, who used the ordinary wet-plate process of his time, are not inferior in definition or in detail to any which have been published in recent years. Fedderson's researches are indeed worthy of all admiration. He used a concave mirror giving excellent images when driven at a speed of one hundred revolutions a second. The velocity was under regulation to within two per cent, and the millionth of a second represented not merely an appreciable distance upon the negative; it was an *easily measurable* quantity.

More than thirty years ago this German physicist stood as Wheatstone had done nearly half a century before him, in the very gateway of the domain in which such activity has shown itself of late,— the domain of electrical resonance. He was the discoverer, along experimental lines, of the oscillatory discharge and the demonstrator of the existence of effects which had already

[1] Foucault; Recueil des travaux scientifiques, Paris, 1878

[2] Michelson; Proc. A. A. A. S., 1879; also Papers of Amer. Ephemeris, Vols 1 and 2, 1882.

[3] Newcomb; Astro. Papers of Amer. Ephemeris, Vol. 2.

[4] Fedderson; Beitrage zur Kenntniss des electrischen Funkens, 1857; also Pogg. Ann., 103, 113, 116 (1859 to 1862).

[5] Rood; Am. Journal of Science, Vol. 2 (111), p. 160.

[6] Trowbridge; Am. Journal of Science, Vol. 42 (111), p. 223.

[7] Boys; Philosophical Magazine, Vol. 30 (5), p. 248.

[8] Fedderson; Pogg. Ann., 116, p. 132.

been embodied in the analytical work of Helmholtz, Thomson and Kirchhoff and he anticipated also many of the discoveries of later investigators and worked out quantitatively the dependence of the rate of oscillation upon capacity, induction and resistance. Two of Fedderson's photographs have been brought to general notice by reproduction in the fourth volume of Wiedemann's Handbook.[1] There is another set which I consider even more significant, showing the increase in the number of oscillations with diminishing resistance. It is copied in fac-simile from the original plate in fig. 1.

Another forerunner in the development of the methods which it is my privilege to consider was Prof. E. W. Blake of Brown University. His results too have become classical; but I refer to them because they are related to later work in ways not always recognized. We are all familiar with his interesting photographs obtained by speaking into the mouth-piece of a Bell telephone,[2] to the diaphragm of which was attached a rocking mirror. Records obtained in a variety of other well-known ways, of some of which I shall have occasion to speak, indicate that these photographs do not give a complete trace of the vibrations which go to make up the articulate utterances by means of which they were excited, but the method is of interest in three particulars:—

1. It is one of the earliest attempts to substitute photography for vision in the study of the transient phenomena of the sound wave.

2. It substitutes a moving sensitive plate for the revolving mirror [3]

3. It is a distinct forerunner of the method applied some years later with somewhat better success by Froehlich to the analysis of alternate current phenomena.

Throughout the history of the study of the phenomena of the time-infinitesimal we find the tendency to be to supplant visual methods by methods of photographic record. One of the most noteworthy achievements in experimental acoustics, for example, is the application of the manometric flame to the study of sound waves. The drawings made by Koenig,[4] in illustration and verifi-

[1] Wiedemann; Die Lehre von der Elektricität. 4, p. 173.

[2] Blake; Am. Journal of Science, Vol. 16 (3), p. 57.

[3] Stein, in a paper cited by Professor Blake (Pogg. Ann. 159, 1876), described a similar device, but it is difficult to ascertain from his paper to what extent he succeeded with his experiments.

[4] Koenig; Annalen der Physik, Vol. 122, p. 666. (Also in his "Experiences d'Acoustique," pp. 47-84.)

cation of the phenomena of the organ pipe and of the analysis of complex sounds, have been admired by all of us; and the repetition of his experiments has delighted an entire generation of demonstrators in physics. In how many minds the question of the feasibility of photographing the manometric flame has arisen I do not know; but quite recently it has been shown by Doumer,[1] and independently by Ernest Merritt [2] in a paper read before this section, that by surrounding the sensitive flame with a mantel of free oxygen (after the method of what was once known as the *Budde* light), sufficient actinic intensity may be obtained to ensure an excellent photographic record on a rapidly moving plate. The results of such photographs applied to the analysis of vowel sounds give evidence of the extraordinary fidelity of the sketches published by Koenig. They also afford a basis for the study of timbre of the sounds to which they correspond, which is open to one objection only, viz., to the uncertainty as to the influence of the inertia of the diaphragm, upon the character of the image. Of this source of error, I shall have more to say in connection with some other researches.

Other interesting examples of the study of the time-element might be drawn from this field. Indeed the science of sound is of necessity largely made up of such work. The beautiful photographs of vibrating strings by Menzel and Raps,[3] which are so fitting an appendix to the earlier labors of Helmholtz,[4] may serve to illustrate the usefulness of the method of photography on a moving plate.

In the study of periodic phenomena two distinct methods of investigation have been established. The first of these consists in the isolation of a desired element of the cycle at each repetition for as long a time as may be necessary to obtain a satisfactory record of the existing conditions. By the selection successively of many neighboring elements we get in this way at last the data from which to construct a complete diagram of the cycle.

This principle has been most fruitful in enabling us to analyze periodic processes not easily approachable by more direct means. The most notable application has been that which is commonly

[1] Doumer; Comptes Rendus, 103 and 105.
[2] Merritt; Proc. A. A. A. S., 41, p. 82. Also Physical Review, Vol. 1, p. 166.
[3] Krigar-Menzel and Raps; Annalen der Physik. N. F. 44 (1891), p. 623.
[4] Von Helmholtz; Die Tonempfindengen, p. 137.

spoken of as the "method of instantaneous contacts," well known to the student of alternating current phenomena.

It is to Joubert[1] (1880), that we owe this ingenious adaptation of the device of properly timed repetitions of instantaneous observations of periodic phenomena (a principle which underlies the phenakistoscope and similar well-known instruments). He made use of it in the study of the changes of potential in the circuit of the alternating current dynamo and between the terminals of the Jablochkoff candle.

In the same year the method was discovered independently and applied to the study of the Brush arc-lighting dynamo, by B. F. Thomas.[2] Joubert pointed out the method of using the quadrant electrometer in alternating circuits, also that the galvanometer might be utilized. ["On peut mesurer cette intensité par l'electrometer mais on peut aussi employer le galvanometre puisque les contacts successive correspondent toujours a une meme phase du courant."] He discovered the retardation of phase in the current curves of the alternating dynamo and the peculiar distortion of the curves in the circuit containing an arc lamp; a matter more fully investigated at a later day by Tobey and Walbridge.[3] Thomas during this period first in the history of the Joubert method used a ballistic galvanometer and condenser.

The periodic phenomena of the alternating current circuit have been among the most important to which the study of the time-element has been applied, and it is to the method of instantaneous contacts that we owe much of the progress of the last thirteen years. It is interesting to note the extension of this method in the study of a variety of allied phenomena. After the publication of Joubert's papers the method seems to have come into common use in the physical laboratories, particularly in the exploration of the fields of continuous current dynamos and motors.

[1] Joubert; Sur les Courants alternatifs et la force electromotive de l'arc electrique. Comptes Rendus, 91, p. 161, July 19, 1880.

[2] Henry Morton and B. F. Thomas; Observations on the electromotive forces of the Brush Dynamo-Electric Machine. (Title only.) Proceedings A. A. A. S., Vol. 29, p. 277 (1880). Professor Thomas gave the results obtained and described the method eleven years later in a communication to the Institute of Electrical Engineers, entitled "Notes on Wiping Contact Methods for Current and Potential Measurements." Trans. of the American Institute of Electrical Engineers, Vol. 9, p. 263.

[3] Tobey and Walbridge; Investigations of the Stanley Alternate Current Arc Dynamo Trans. Am. Inst. Electrical Engineers, Vol. 7, p. 367.

In 1888 it was applied by Duncan, Hutchinson and Wilkes[1] to the study of induction coils and transformers. To them we owe the first set of complete diagrams relating to the performance of this class of alternating current apparatus. In the same year Meylan[2] used an interesting modification of the method in the investigation of the vibratory magnetic call-bell of Abdank.

In the same year appeared the first definite data with reference to the Westinghouse alternating dynamo, at the hands of Messrs. Searing and Hoffman[3] of Stevens Institute. Then followed in the order named, the researches of Ryan and Merritt,[4] Humphrey and Powell,[5] Tobey and Walbridge,[6] of Marks,[7] of Herschel,[8] of Fortenbaugh and Sawyer.[9]

In all these investigations the methods under consideration have been used with varying accessories in the problem of the transformer.

In 1890 it was applied under much more difficult conditions to the analysis of "ball and point effect" by Archbold and Teeple.[10]

In 1891 Thompson[11] determined the intricate changes of induction in open coil arc lighting machines by means of the same method, and Ryan[12] utilized it in his investigations of the influence of the air gap upon the performance of dynamos and motors. In 1892 Duncan[13] described modifications of the method of instantaneous contacts by means of which the rapidity of reading is greatly enhanced.

[1] Duncan, Hutchinson and Wilkes; Experiments on Induction Coils. Electrical World, Vol. 2, p. 160, 1888.

[2] Meylan; Sur les Appels Magnetiques. La Lumière Electrique, 27, p. 220, 1888.

[3] Searing and Hoffman; Variation of the Electromotive Force in the Armature of a Westinghouse Dynamo. Journal of the Franklin Institute, Vol. 123, p. 93.

[4] Ryan; Transformers. Transactions Am. Inst. Electrical Engineers, Vol. 7, p. 1, 1889.

[5] Humphrey and Powell; Efficiency of the Transformer. *Ibid.*, Vol. 7, p. 311.

[6] Tobey and Walbridge; *Ibid.*, Vol. 7, p. 367.

[7] Marks; *Ibid.*, Vol. 7, p. 324.

[8] Herschel; *Ibid.*, Vol. 7, p. 328.

[9] Fortenbaugh and Sawyer; *Ibid.*, Vol. 7, 334.

[10] See Nichols; On Alternating Electric Arc between a Ball and Point. Am. Jour. Science, Vol. 41, 1.

[11] M. E. Thompson; Study of an Open Coil Arc Dynamo. Trans. Am. Inst. Electrical Engineers, Vol. 8, p. 375.

[12] Ryan; Relation of the Air Gap and the Shape of the Poles to the Performance of Dynamo-electric Machinery. *Ibid.*, p. 451.

[13] Duncan; Note on some experiments with alternating currents. *Ibid.*, Vol. 9, p. 179.

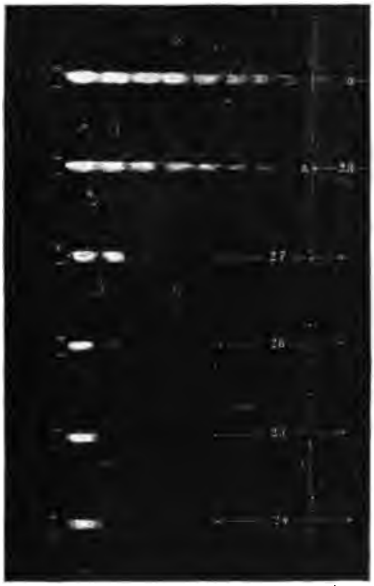

**Fig. 1.**

**Fig. 2.**

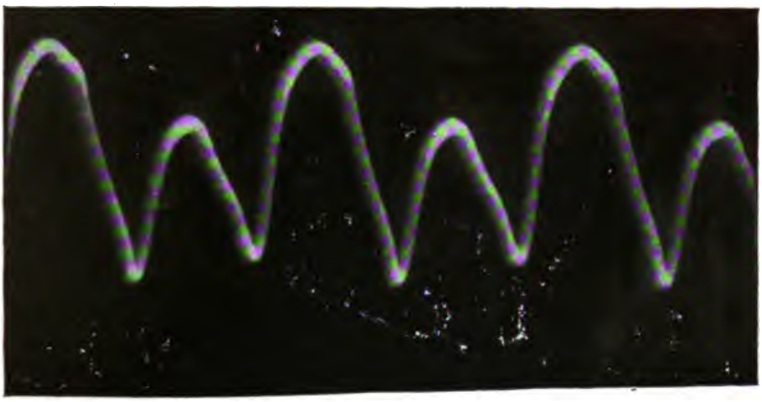

**Fig. 3.**

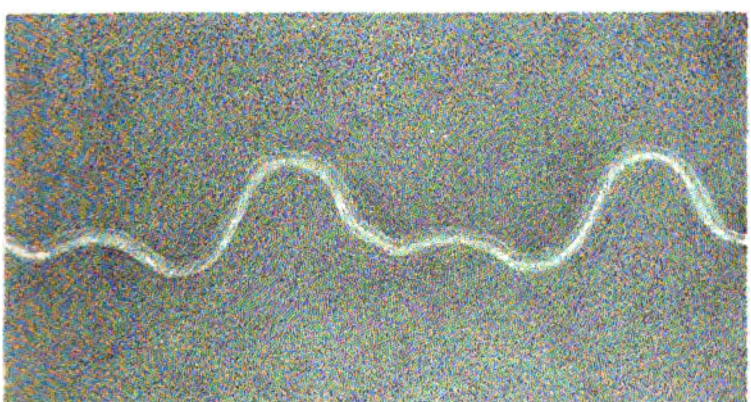

**Fig. 4.**

FIG. 6.

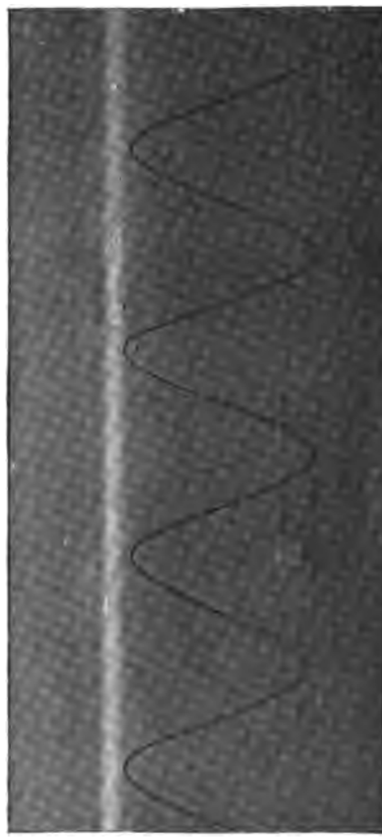

FIG. 7.

During the present meeting, you will doubtless have the pleasure of listening to a description of the applications of the same device to the study of electrostatic hysteresis.[1]

Such has been in brief the history of a method by means of which in greater degree than of any other we have been able to extend and complete our knowledge of alternating current phenomena.

To the practical electrician and to the theorist alike, the domain has been one of the most attractive of those which have been developed in recent years. To the electrical engineers of the younger generation the very complexity of alternate current theory has proved a benefit. It has forced them to increased mathematical proficiency and to more rigorous thinking; it has indeed served as an excellent source of discipline. What the problems of submarine telegraphy did for the English electricians who served their apprenticeship during the early days of the cable-laying industry, compelling the development of those sturdy qualities, which have been so highly serviceable in every branch of electrical progress since, the intricacy of alternate current practice is unquestionably doing for the younger school which is growing up to-day in this country. The difficulties which have to be met and overcome in this field of work will have an excellent influence upon the manner in which the problems of the future are to be approached.

Another investigation, which owes its existence to a most ingenious application of this same principle of instantaneous contacts periodically repeated, is well known to all of you. I refer to Prof. E. H. Hall's[2] study of periodic heat flow in cylinder walls of the steam-engine by means of thermo-elements embedded within the metal and connected momentarily during a selected time-element in the course of each stroke with a sensitive galvanometer. To my mind no more interesting example of the indirect method of studying the phenomena of the time-element could be found than this suggestive memoir.

This method of instantaneous contacts has been a fruitful one and productive of high results, but it does not yield a knowledge of any individual time-element, nor the picture of any single com-

[1] Reference is here made to the paper presented by Messrs. Bedell, Ballantyne and Williamson; Alternate-current Condensers and Dielectric Hysteresis. Physical Review, Vol. 1, p. 91 (subsequent note).

[2] E. H. Hall; A Thermo-electric Method of Studying Cylinder Condensation in Steam-engine Cylinders. Trans. Am. Inst. Electrical Engineers, Vol. 8, p. 236.

pleted cycle. Numerous attempts to record single cycles have been made, the results of which are of considerable interest.

The device which lay nearest to hand and which by its performance seemed to promise success in this direction was the magneto-telephone. The investigations of Mercadier[1] had already paved the way to some extent, when Froelich described his experiments upon the optical representation of the movement of the diaphragm of the telephone, followed almost at once by Thomson.

Froelich[2] reported his preliminary results to the Electrotechnische Verein of Berlin in 1887. Elihu Thomson[3] brought out his indicator for alternating circuits, an instrument in which the movement of a diaphragm was amplified by levers and then made visible by optical means (or photographed) in the same year. Froelich's method in its complete form, including the photography of the images from the revolving mirror,[4] was first described in the year 1889. Some of the curves, published in the papers just cited, and particularly the experiments shown in the exhibition of the method at the Frankfort Electrical Exposition of 1891, are most striking; but considering the method by which they are produced, the question inevitably arises as to the part played by the inertia of the moving masses.

Froelich himself points out the necessity of great care in the matter of the adjustments and the difficulty of distinguishing the natural oscillation of the plate from the forced vibration which it is the object of his method to observe. Some experience with Froelich's method has convinced me that not only is extraordinary skill necessary in order to obtain, by means of a mirror attached to the diaphragm of a telephone, curves which should represent even with a fair approximation the law of whatever periodic changes we may desire to record, but that the attainment of the proper adjustment is a matter so entirely fortuitous and its maintenance so uncertain as to deprive the method of much of its usefulness. One may indeed hope to get, by means of successive adjustments, curves which correspond to a known type, but whether in passing to new

[1] Mercadier; Journal de Physique, Vol. 9, pp. 217 and 282.

[2] Froelich; the Optical Representation of the Movements of a Telephone Diaphragm. La Lumière Electrique, Vol. 25, p. 180 (1887).

[3] E. Thomson; An Indicator for Alternating Circuits. La Lumière Electrique, Vol. 27, p. 339 (1888).

[4] Froelich; Ueber eine neue Methode zur Darstellung von Swingungskurven. Electrotechnische Zeitschrift. Bd. 10, pp. 345, 369 (1889).

and unknown types the apparatus retains its faithfulness, is always a question.

By way of illustration, I introduce three of an extended series of curves obtained by this method with a telephone in circuit with an alternating current dynamo. The character of the cycle had been determined by the method of instantaneous contacts. The true cycle was represented by a curve of sines, but with the apparatus under consideration complex curves of the kinds shown in figs. 2, 3, 4, were the rule; curves even approximating to simple sinuosity were the rare exception.

The difficulties of the method lay not merely in the tediousness of adjustment, but rather in the tendency to revert to complex forms under changes of condition so slight as to be entirely beyond control. The remedy clearly consists in the elimination of mechanism and in reducing the inertia of the moving parts. At the suggestion of an assistant, Mr. E. F. Northrup, I tried the following experiment:—

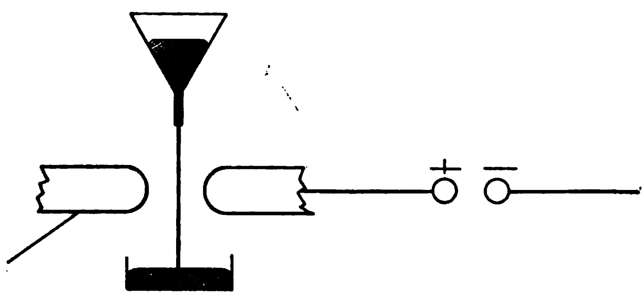


FIG. 5.

A mercury stream flowing from the contracted nozzle of a funnel (fig. 5) was made to pass between two metal terminals which were attached to the poles of a large Holtz machine. A portion of the falling column of mercury within the electrostatic field was illuminated by means of an arc lamp and so much of it as could be seen through an horizontal slit was photographed by transmitted light. The sensitive plate was given rapid vertical motion through the field of the camera. When the machine was out of action there resulted a vertical trace running the length of the developed plate. As soon as the machine was put into operation, deflection of the mercury stream occurred. It was the object of the experiment to determine the performance of the stream under the sudden fluctua-

tions of the field which occurred when the Holtz machine was under rapid discharge.

Fig. 6 is from a photograph taken when nearly one hundred sparks a second were passing between the poles.

The photograph from which fig. 7 is drawn was obtained in a similar manner, the deflecting forces, however, being due to the action between the lines of a stationary magnetic field and those of an alternating current traversing the mercury column. The arrangement of the apparatus is shown in fig. 8. The mercury stream was introduced into the circuit of the alternating current dynamo, already made use of in the experiments upon Froelich's method. It flowed through a strong magnetic field with horizontal lines. The transverse oscillations of the mercury under these conditions were very apparent. When photographed by means of

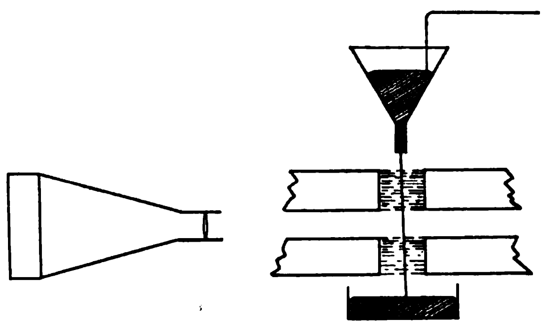

FIG. 8.

a camera with optical axis parallel to the lines of force, the stream strongly illuminated from behind and viewed through a narrow horizontal slit as in the previous experiment, the sinusoidal trace indicated in fig. 7 was obtained. All the complexities of the telephonic trace disappeared in these records and curves corresponding to those of the method of instantaneous contact were always produced. The experiment was made by Mr. Henry Floy, to whose efforts the photographs by Froelich's method are also due. This method has not been further developed. I introduce it here to show that increased accuracy of record may be looked for as the result of reducing in any practicable manner the mass of the indicating device.

Another attempt to record single periods in dynamo-electric work

should be mentioned here. It is described by Moler[1] in a recent paper. By means of a D'Arsonval galvanometer with a period of vibration of a few thousandths of a second, curves of varying potential are traced which show excellent agreement with measurements by the method of instantaneous contacts. The instrument is not free from the errors due to inertia. It is reliable only in recording changes of period considerably greater than its own, but its use is a step in a direction along which progress may be looked for.

Thus far I have dealt with methods of studying periodic changes, the time-elements of which are easily within reach through experimental devices. I might have devoted myself with as good reason to the consideration of recent advances in the study of electrical oscillations of a higher order of frequency. This is a department of physics in which much has been done of late, but so much has been written at second hand as well as in the way of original treatment, that further reiteration is uncalled for. The work of Hertz and of his hosts of followers is familiar to us all. In the study of electrical oscillations even of very high frequency, photography has been used with success and details of the phenomena of time-elements truly infinitesimal have been secured. By the labors of Wiener,[2] waves of a still higher order than those which have occupied the attention of the electrician have been photographed and a new field of the greatest promise has been thrown open to the optician. The isolation of a single light vibration may indeed still be as far from us as is the inspection of the molecule by means of the microscope, but in the meantime we have in the photography of a system of standing light waves, an achievement well worth celebrating.

In the investigation of the phenomena of the time-infinitesimal, so far as periodic changes are concerned, we see that the experimenters of the present time are gaining much of detailed knowledge. There is another field equally important in my opinion which is as yet for the most part unexplored. The study of the beginnings of changes brought about by abrupt shifting of the conditions equilibrium is one from which very much may be expected. Al-

[1] G. S. Moler; A Dynamo-Indicator, or Instantaneous Curve-writing Voltmeter. Trans. Am. Elec. Engineers, Vol. 9, p. 223.

[2] Wiener; Stehende Lichtwellen und Swingungsrichtung des polarisirten Lichtes. Annalen der Physik N. F., Vol. 40, p. 203.

ready suggestive beginnings have been made, but the researches have not been pushed to the limit of the experimentally possible. Oftentimes interesting observations of what might be termed "starting phenomena" have been recorded, but quantitative results are lacking. Take, for example, the brilliant work of Becquerel[1] with the phosphorescope. What a mass of fascinating and suggestive material that savant has gathered into the first volume of his book on light![2] What a world of interesting material these preliminary observations present to him who shall undertake to determine quantitatively, wave-length by wave-length, the changes which the radiations from the numerous luminescent materials undergo, beginning with the instant of exposure and following the vanishing light until it is gone!

Of a few isolated cases which have been forced upon us by their practical importance, we have some complete knowledge already. With the phenomena in cables when current is suddenly introduced or circuit is broken, we are reasonably familiar. The case of the charge and discharge of condensers has been treated analytically under assumptions the precise truth of which is still to be verified. The detailed study by experiments carried to the utmost refinement, of the very cases which seem to have been most completely covered by theory, is especially important; since in this way only can the assumptions upon which our analysis is based be rigorously determined and the necessity of modifications be ascertained. For some of this work methods already in use in the study of periodic phenomena will suffice. The curve-writing voltmeter, for example, may be made to give records running to within a thousandth of a second of the instant when a process such as electrolysis, electrolytic polarization, voltaic action or the charge and discharge of a condenser begins. Instruments such as the von Helmholtz pendulum, for the isolation of definite small time-intervals, may also be applied to a great variety of progressive phenomena, enabling us to approach by successive steps almost to the very beginnings of the changes to be analyzed. Concerning known methods let me point out in conclusion that photography with the moving plate is a means, the limitations of which have not yet been discovered. It

[1] Becquerel; Comptes Rendus, 96, pp. 121, 1215, 1853.
[2] Becquerel; "La Lumière," I, pp. 206-422.

is equally applicable to periodic and to progressive phenomena, often with results of unexpected beauty and significance.[1]

The remarkable experiments of Mach[2] and of Boys[3] indicate that the dry plate is still abundantly exposed within intervals so short that the swiftest of modern projectiles give images as of a body at rest. The laws of electrical resonance have already been so far determined that we can construct condensers, the duration of the discharge of which is a matter of computation and the precise moment of the discharge of which after a given event is quite within control. This single device, consisting of the exposure of the photographic plate by means of a properly timed spark, brings under observation a set of time intervals of a new and higher order of brevity. Much is destined to be learned by means of it concerning the nature of matter, and much more I think, from other, possibly still more powerful, methods which will doubtless be developed when the importance of the study of the time infinitesimal is more generally recognized.

[1] In photographing the alternate current arc a single exposure of a continuous current lamp upon the moving plate by way of check brought out the seat and precise nature of the hissing of the arc in a manner scarcely to be reached in any other way. For the method used see "A Photographic Study of the Electric Arc. Trans. Am. Inst. Electrical Engineers," Vol. 8, p. 214, 1891.

[2] Mach; Wiener Sitzungsberichte, 95, p. 764, also 97, p. 41.

[3] Boys; On Electric Spark Photographs, etc. Nature, Vol. 47, p. 415.

# PAPERS READ.

IRREGULARITIES IN ALTERNATE CURRENT CURVES. By FREDERICK BEDELL, K. B. MILLER and G. F. WAGNER.

[ABSTRACT.]

CURVES of a more or less sinusoidal nature are commonly plotted to represent the instantaneous values of an alternating current, and various conclusions are drawn from their forms and relative positions. For any perfectly symmetrical generator the curves for successive periods are the same, both in shape and size, but decided differences may exist between the successive portions of the curves for machines which are not symmetrical. In the larger machines this difference is not likely to exceed one or two per cent, not introducing serious error into the mean results, although occasionally a greater difference may be found. In the smaller machines, however, the instantaneous curves show marked irregularities, so that to take one period as typical is not always justifiable.

To investigate this point experimentally, curves were taken from three small eight-pole Westinghouse alternators, showing the instantaneous values of the electromotive force. The electromotive force was measured by means of a Thomson multicellular voltmeter connected around one of two incandescent lamps connected in series across the terminals of the machine, the whole electromotive force being readily found by a proper calibration. The instantaneous reading was obtained by means of a revolving contact-maker on the shaft which made the contact at a definite point in the revolution of the armature, as indicated by a graduated disk. The three machines from which curves were taken were apparently alike in all respects. An inspection of the curves shows a decided difference in the curves from different machines, and also differences between successive periods of the same machine. This is further shown by comparing the areas of consecutive loops of the curves. The resistance used was non-inductive, and whether the curves represent current or electromotive force is merely a question of scale. In any case, the ordinates of the current curve represent the instantaneous values of the current, $i = \frac{dQ}{dt}$, whence the area inclosed by the curve is $\int idt = Q$; that is, the area inclosed by the current curve is proportioned to the quantity of electricity which flows and for a complete cycle or revolution of the armature must be zero inasmuch as there is no continual flow one way or the

other. Likewise, the ordinates of the electromotive force curve represent the instantaneous values of the electromotive force, $e = \frac{dN}{dt}$, and the area enclosed is proportional to the change in the number of lines of magnetization through the armature. Evidently the algebraic sum of these areas must be zero for a complete cycle, since the magnetization is the same at the beginning and end of a complete revolution of the armature. To establish this point experimentally, curves from two machines were taken, each for a complete revolution of the armature. In these curves, ordinates represent electromotive force and abscissæ the portion of the cycle, with reference to the position of the armature at which readings were taken. The areas of these curves were obtained by a planimeter and in arbitrary units are

| Areas, Curve No. 2. | | Areas, Curve No. 3. | |
|---|---|---|---|
| Positive. | Negative. | Positive. | Negative. |
| 3.71 | 3.61 | 3.51 | 3.50 |
| 3.98 | 3.59 | 3.47 | 3.47 |
| 3.58 | 4.35 | 3.68 | 3.60 |
| 3.25 | 3.27 | 3.57 | 3.67 |
| 14.52 | 14 82 | 14.23 | 14.24 |
| Total | —.30 | Total | —.01 |

Although, separately, the positive and negative areas widely differ, the sums of the positive and negative areas are very nearly equal, thus experimentally establishing the conclusion reached above.

[Printed in full in the Physical Review, Vol. 1, No. 3, Nov.–Dec., 1893.]

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ALTERNATE-CURRENT CONDENSERS AND DIELECTRIC HYSTERESIS. By FREDERICK BEDELL, N. F. BALLANTYNE and R. B. WILLIAMSON, Ithaca, N. Y.

[ABSTRACT.]

THE following investigation was undertaken in order to determine the behavior of condensers when used on alternating current circuits, particular attention being given to hysteresis in the dielectric.

The condensers, six in number, upon which the experiments were performed are intended to be used commercially on 500 volt circuits. The plates are tinfoil, the useful part of which is of the following dimensions: length, 10½ inches; width, 8 inches; thickness, .0007 inch. The dielectric is of waxed paper .0043 inch thick. There are sixty-five sheets of tin-foil (total) per slab and two of these slabs are placed together in one tin case.

The capacity of each was measured by Thomson's method of mixtures. The capacities did not differ much from one and a half microfarads. The

capacities were also measured by an alternating current method, the formula used being $C = \frac{I}{E\,\omega}$ in which $I$ is either the maximum or mean square value of the current in the condenser when $E$ is the corresponding maximum or mean square value of the harmonic impressed electromotive force, and $\omega$ is $2\pi$ times the frequency. A Thomson milli-ampere balance and multicellular voltmeter were used to measure the current and electromotive force respectively. The results given by this alternating current method indicate that, for practical purposes, it may be used where apparatus for more refined measurements is not available.

It was found that the capacity depended somewhat on the temperature. In one case the capacity changed very little between 0° and 30° C. Above 30° the capacity decreased until a temperature of 40° was reached, beyond which the capacity increased rapidly with increase of potential.

The leakage and dielectric resistance were determined by charging the condenser to a potential of about 700 volts and allowing it to stand several hours with its terminals connected to a multicellular voltmeter by means of which the potential could be read at any time. The leakage was at first greater and afterwards less than that given by theoretical considerations. The dielectric resistance apparently increases very greatly with increase of time and decrease of potential, owing probably to the soaking in effect.

Some idea of the loss that occurs in a condenser when used on an alternating current-circuit was obtained by finding roughly the rates of heating of the condenser. With a harmonic electromotive force of 500 volts at which potential the current was about three-fourths of an ampere when the frequency was 160, the rate of heating was about 1° C. an hour.

The three-voltmeter method of measuring power was employed for the same purpose. Both methods showed the loss to be small.

An attempt was made to obtain an hysteresis loop for a condenser by plotting curves showing the relation between the quantity of charge and increasing and decreasing potentials. If the curves drawn for increasing and decreasing potentials enclosed an area, it would indicate an hysteresis loss. No area was discernible. As the results of later experiments show the presence of hysteresis, it is probable that the time element had some effect. This was unavoidable in the method used in this case.

The behavior of condensers subjected to an alternating electromotive force was further studied by plotting instantaneous curves for electromotive force and current. From these curves the power expended at any instant could be determined and a watt curve drawn showing its variation. The power wasted and the efficiency were measured from the watt curve. In one run with an harmonic impressed electromotive force of about 500 volts at a frequency of 140, the loss was calculated to be 4.4 watts which makes the efficiency 96.9%. In a similar preliminary run the loss was found to be 2.47 watts corresponding to an efficiency of 98.05%.

The method devised for obtaining an hysteresis loop from the instantaneous curves for current and electromotive force is as follows: Since the quantity of electricity that flows through a circuit in the time $dt$ is $idt$, if we start with no charge in a condenser and measure the value of the current from instant to instant, the total charge in the condenser at any time may be found by summing up the instantaneous increments of charge. The total quantity of charge at the time $t$ is $q = \int_o^t idt$, and is represented by the area enclosed between the *X-axis*, the current curve, and ordinates drawn where $t = o$ and $t = t$. Between two successive points where the current is zero, the condenser becomes discharged, and then charged in the opposite sense, so that the area of one of the loops, enclosed between the current curve and the *X-axis*, represents double the maximum charge. By the measurement of these areas, the quantity of charge in the condenser for different potentials was found. An hysteresis loop was obtained by plotting potentials as abscissæ and quantities of charge, obtained in the way described, as ordinates. The area of this loop is a measure of the amount of energy dissipated in the condenser per cycle, being equal to $\int vdq$. The area, measured by an Amsler planimeter, was found to be equivalent to $.0512 \times 10^7$ ergs. As there were one hundred and forty complete alternations per second, the rate of dissipation of energy in the condenser is $.0512 \times 10^7 \times 140 = 7.17 \times 10^7$ ergs per second $= 7.17$ watts.

In alternate-current circuits the neutralization of the effects of self-induction and capacity opens a large field for work. How far this neutralization can be effected depends upon the nature of the impressed electromotive force and the condenser itself. With a perfect condenser and an harmonic electromotive force, it would be possible to annul completely the effect of self-induction. With an alternating current which is not harmonic and a condenser which dissipates a certain amount of energy, *i. e.*, is not perfect, complete neutralization of self-induction can only be approximated. Experiments were made with condensers in question, and curves obtained showing the variation in the current for changes in capacity for certain circuits. These curves showed a maximum point for the critical value of the capacity, but this was not as high or as well marked as in the theoretical curves[1] plotted on the supposition that the condenser was perfect and the electromotive force harmonic.

In this paper we have given the results of an investigation of the loss in the dielectric of a condenser for one particular frequency. A complete investigation of the subject of dielectric hysteresis would include the determination of this loss for different frequencies and potentials, and also for different dielectrics, thus furnishing data upon which to establish the law for dielectric hysteresis.

[Printed in full in the Physical Review, Vol. 1, No. 2, Sept.-Oct., 1893.]

[1] Alternating Currents, by Bedell and Crehore, p. 139.

NOTE ON THE USE OF A ROTATING SECTORED DISC IN PHOTOMETRY. By ERVIN S. FERRY, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

ONE source of difficulty in the photometric comparison of lights of very different intensity is that the photometer must be used in that part of the bar where slight errors in observation produce large errors in the result. The method usually employed to obtain the reading toward the middle of the bar is to interpose in the path of the beam of light from the intenser source a rotating disc, having sectors removed in known proportion to the area of the whole disc. The ratio of the light that passes through, to the whole light, should then be the same as the ratio of the open part of the disc to the whole disc.

Tests now in progress to test the validity of this assumption have developed the following facts:

I. While it is physically true that the proportion of light transmitted by a rotating sectored disc to the total incident illumination, is equal to the ratio of the total aperture of the disc to the entire disc, yet the effect of this light upon the retina will not always be proportioned to the ratio of the total aperture of the disc to the entire disc.

II. With mixed light containing elements of different luminosity shining upon the retina, a rotating sectored disc will appear not to cut off all the elements in equal proportion but will intercept most strongly the elements of low luminosity.

III. With any given light, the error introduced by the use of the rotating sectored disc increases as the aperture of the disc diminishes.

IV. With ordinary illuminants the error is negligible when the total aperture of the disc is more than one-half the entire disc, but rapidly increases as this aperture is diminished.

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ON PHYSICAL ADDITION OR COMPOSITION. By ALEXANDER MACFARLANE, University of Texas, Austin, Tex.

[ABSTRACT.]

THE paper investigates what may be called generalized addition. It treats of the addition of masses at different points, the addition of forces applied at the same or different points, the composition of finite rotations round axes which may or may not intersect, and finally applies the same method to the composition of finite screw motions.

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THE ELECTRIC STRENGTH OF SOLID, LIQUID AND GASEOUS DIELECTRICS. By Prof. A. MACFARLANE and Mr. G. W. PIERCE, University of Texas, Austin, Tex.

[ABSTRACT.]

THE paper contains an account of measurements made of the electric strength of paraffined paper and of beeswaxed paper, and of kerosene oil,

also a discussion of these results taken along with those previously obtained by Macfarlane and Steinmetz.

[Printed in full in the Physical Review for November, 1893.]

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FATIGUE IN THE ELASTICITY OF STRETCHING. By Dr. JOSEPH O. THOMPSON, Haverford, Pa.

[ABSTRACT.]

THE phenomenon of elastic fatigue in the elasticity of torsion was observed by Lord Kelvin twenty-eight years ago, but up to the present time no one has observed this phenomenon in stretch-elasticity. The experiments described in the above paper were made in the spring of '91 in the Physical Institute of the University of Strasburg and show clearly an elastic fatigue, though it is not so great as in the case of torsion. The stretch-modulus was in no case affected by it by as much as $\frac{1}{2}$ %.

[Printed in full in the Physical Review for Jan. and Feb., 1894.]

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ON SO-CALLED NEGATIVE LIGHTNING, WITH ILLUSTRATIONS. By Prof. W. LECONTE STEVENS, Rensselaer Polytechnic Institute, Troy, N. Y.

[ABSTRACT.]

A BRIEF discussion is given of the forms manifested by the lightning flash, and of the difference between the conventional representation and that obtained by aid of the camera. This is illustrated by the projection of several lantern slides. Two of these reveal *black* branches sent forth from the main lightning stem. The explanation of this is then considered by discussing two hypotheses:

1. That of chemical reversal on the photographic plate.
2. That of atmospheric absorption.

The writer gives his preference to the second of these hypotheses. The phenomenon is one whose existence could not have been learned except by photography.

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AN AUTOMATIC TOEPLER PUMP. By Prof. EDWARD W. MORLEY, Cleveland, Ohio, Station B.

[ABSTRACT.]

THE paper describes the principle of a mercurial air-pump operated by water pressure which has been found very convenient in use and certain in action. Its special advantage is, that the mercurial pump may be placed anywhere in a building, and may be moved about as readily as an ordinary pump; while the air-compressor is permanently fixed in a suitable position in the basement of the building.

[Printed in full in the Am. Jour. Sci., 1894.]

### On the effect of evaporation upon the relative dimensions of bars of metal partially submerged in water. By Prof. Wm. A. Rogers, Waterville, Me.

[ABSTRACT.]

This paper describes the experiments which have been made to determine the temperature of the air over that of a body of water exposed to air for about 80° and for 64° Fahr.

Secondly, a similar investigation is made of the effect of evaporation upon the length of a bar, submerged in water, but with one surface exposed to the air. The general result of this investigation is that comparisons of standards of length made in the usual way are subject to a constant error corresponding to a change through 0.15° Fahr.

The paper also gives a résumé of the results obtained in previous investigations of the two bars compared.

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### An apparatus for the generation of oxygen and hydrogen by electrolysis. By Prof. Edward L. Nichols and George S. Moler, Ithaca, N. Y.

[ABSTRACT.].

The apparatus described is in use at Cornell University, where gas is supplied by a system of storage tanks and pipes for use in various lecture rooms and laboratories. After indicating the difficulties of using various forms of the electrolytic cell, with and without porous diaphragms, and of maintaining in constant service electrodes of platinum, iron, lead and carbon, the present very successful form is described. The essential features are:

(1) Very deep glass jars with electrodes inserted separately in long vertical tubes.

(2) The use of platinum electrodes upon the oxygen side.

(3) The use of lead electrodes upon the hydrogen side.

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### On the continuous spectrum of the alkalies. By Prof. B. W. Snow, Madison, Wis.

[ABSTRACT.]

Metallic sodium was burned in the flame of a Bunsen burner, and the radiation emitted was examined by means of the spectro-bolometer.

The greater part of the energy in the visible spectrum occurred in the strong D line, which was superposed upon a faint continuous spectrum. The character of the radiation in the infra-red was found to be that emitted by an incandescent solid. An oxyhydrogen flame was then substituted

for the Bunsen burner when it was found that under the influence of the higher temperature, the intensity of the D line was greatly increased, but that the energy in the infra-red had almost entirely disappeared. It would seem therefore by this, that the incandescent metallic oxide, with which the flame of the Bunsen burner was filled, was dissociated by the higher heat of the oxyhydrogen flame.

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ELECTROLYTIC POLARIZATION. By JOHN DANIEL, Adj. Prof. Physics, Vanderbilt University, Nashville, Tenn.

[ABSTRACT.]

USING a voltameter with platinum electrodes, separated by a glass partition bored in the centre with a hole two centimetres in diameter, over which was sealed a smaller glass plate bored with a hole one and one-half centimetres in diameter, this smaller hole being covered by metal plates of various thicknesses sealed tight over it, a study has been made of the polarization phenomena upon these thin metal partitions in different electrolytes and under various conditions as to thickness of partition, current strength, temperature, etc.

Without now going into details of the apparatus, methods, and results, the following summarized statement may be interesting:

1. The polarization on a gold-leaf partition in good-conducting $H_2SO_4$ is zero, or too small to detect with our apparatus for the range of current used.

2. The "critical thickness" in good-conducting solutions of $H_2SO_4$, $CuSO_4$, and NaCl is *greater* than .00009 millimetres for gold; .00015 millimetres for platinum; and .0005 millimetres for aluminum, under the above conditions. It is *less* than .0004 millimetres for gold; .002 millimetres for platinum; and .002 millimetres for silver.

3. The "upper critical limit" of thickness under these conditions seems to be about .004 millimetres, rather less than No. 3 gold.

4. Tables I, II, and III all point to the conclusion that, between "critical limits" of thickness, the polarization for a given current increases with the thickness.

5. Table II, showing relation of polarization to current, expresses two interesting facts: (*a*) that the polarization on "thick" plates is about the same, in this voltameter, for all currents between .2 ampere and, say, .01 ampere, provided time enough be allowed in each case for the current to become constant, *i. e.*, between the upper limit of current, at which the development of gas is so profuse as by mechanical obstruction and irregular escape to interfere, and the lower limit, at which the formation of gas is no faster than it can be dissipated. (*b*) Quite different is the case for "thin" plates where, within the limits of current and thickness prescribed, the polarization is dependent upon the current and gives for each thickness a different curve, or rather straight line, for they are all straight lines converging to the origin, and differing only in *slope*. The current strength, at which the polarization on very thin plates would reach

a maximum, is far above that used, being, perhaps, expressed in amperes instead of tenths and hundredths.

By *thick* plates are defined those above the "upper critical limit;" by *thin* plates, those below this limit of thickness.

6. Inspection of Table III, which gives the time-change of the polarization, will show a similar distinction between "thick" plates and "thin" plates, as was noted in the last paragraph, viz., that for thick plates the change is considerable and continues slowly for hours; for thin plates, the change of polarization with time is both less pronounced and extends over much less time.

7. It was noted, especially in the case of $CuSO_4$ as electrolyte, that there was polarization on golf-leaf if the gold exposed came in contact with the solution some distance beyond the edge of the hole in the glass plate to which it was sealed: thus in $CuSO_4$, for the stronger currents used, there was a symmetrical deposit of Cu, decreasing in thickness from the outside toward the centre, and vanishing at a small distance from the edge of the hole, this distance being less the stronger the current. If only one corner was left exposed, the Cu was deposited there. This phenomenon was farther tested by bending a thick strip of aluminum, 4 centimetres long, into the shape of a narrow U, and simply hanging this U in the *open* hole of the glass partition, in $CuSO_4$, and closing the circuit on the voltameter; the two ends of the metal strip being thus in contact with the $CuSO_4$ on opposite sides of the glass two centimetres from the edge of the opening, there was a decided deposit of Cu on one end and escape of oxygen from the other end.

8. In $CuSO_4$, all the plates except those below the critical thickness were destroyed by oxidation. No. 1 silver was destroyed in less than one minute. Of course, gold and silver above the critical thickness could not be used in NaCl, because of chemical action, though the thinnest plates were quite unaffected. Only the No. 7 gold was tested in KOH, as it dissolved the sealing-wax.

9. Thick plates of gold were strongly oxidized in $H_2SO_4$, especially with strong currents. Thin gold plates were apparently only oxidized under action of strong or long-continued currents. Compare Tables II and III. Silver was even more easily oxidized than gold. Aluminum was so intensely oxidized by the current that no satisfactory measurements could be made for this metal, though the tin foil was unaffected.

10. With $H_2SO_4$ as electrolyte, after a thick plate of pure gold had been used as partition for the time-change of Table III, the end cathode was found to be gilded. A thick Pt plate being then substituted for the gold in the same solution for the results of No. 1 Pt in Table III, the Pt partition was found, on removal, to be gilded. The polarization for No. 1 Pt in this case was somewhat less than for the same Pt after both it and the end electrodes were thoroughly cleansed, the electrodes re-platinized, and fresh solution made.

11. The polarization in $CuSO_4$, using Cu electrodes, reached a maximum almost immediately and remained very constant. The maximum

polarization for thick Pt in $CuSO_4$ was hardly 75 per cent of that for the same in $H_2SO_4$. In NaCl the polarization became constant very quickly also, but its value was decidedly greater, especially on thin plates, than in $H_2SO_4$; though the same distinctive behavior of thick and thin plates was maintained.

12. In $H_2SO_4$ of different concentrations the maximum polarization for a partition was of the same order of magnitude; but its value for very weak currents was decidedly greater in weak solutions than for the same current in stronger solutions, up to 30 per cent. This shows itself especially with thin plates, and also in the shorter time required for thick plates to reach a maximum polarization with weak currents. The greater change in temperature and the greater change in concentration of weak solutions may account for this.

For currents between 0.1 and 0.2 ampere, the polarization on the end electrodes was:

For $H_2SO_4$, 1.84.

For NaCl, 1.98.

For $CuSO_4$, 0.00, with Cu electrodes, though, if the current density was too great or the time long, the anode would oxidize and become irregular. C. Fromme, in a paper, "Ueber das Maximum der galvanischen Polarisation von Platinelektroden in Schwefelsäure" (*Annalen d. Physik u. Chemie*, Band XXXIII, s. 80–126), states that the maximum polarization varies both with the concentration and the relative size of the electrodes, the extreme limits being given as 1.45 to 4.31 volts—the minimum polarization coinciding with maximum conductivity. His method for measuring polarization was somewhat similar to that used in this work. As bearing upon "the change of polarization with time," I would refer especially to the investigation of Dr. E. Root upon this subject, discussed by Professor von Helmholtz, *Wisch. Abh.*, Vol. I, p. 835. These experiments by Dr. Root seem to prove clearly that the liberated ions penetrate deeply into the electrode, even when liberated upon but one side of it, as in this case. I take great pleasure in expressing here my thanks and deep obligation to Prof. A. Kundt and Dr. L. Arons for their kind sympathy and direction in this work.

Using $CuSO_4$ on one side of the partition, and $H_2SO_4$ on the other side, careful determinations have developed the curious fact that, although there is no visible development of ions (neither Cu nor O) at the gold-leaf partition, yet the Cu does not pass through the gold-leaf with the current, but H appears on the cathode instead, provided the current density at the partition be not greater than about .2 ampere per square centimetre.

The "critical current-density" at which the ions just begin to appear visibly on a gold-leaf partition varies for different electrolytes between the limit of 5.7 amperes for 30 per cent $H_2SO_4$ and sensibly zero for lead acetate.

This "critical-density" is proportioned to the conductivity of the electrolyte. It therefore also has a decided positive temperature coëfficient.

[This paper will be printed in full in the Physical Review and in the Phil. Mag.]

SOME APPLICATIONS OF ELECTRIC HEATING IN PHYSICAL LABORATORY PRACTICE. By Prof. EDWARD L. NICHOLS, Ithaca, N. Y. [Printed in full in the Physical Review, 1893.]

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NOTE ON SURFACE TENSION OF LIQUIDS. By E. F. NICHOLS, Hamilton, N. Y. [Printed in full in the Physical Review, 1893.]

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ON THE EFFECT OF TEMPERATURE AND OF ELECTRIC DRIVING ON THE PERIOD OF TUNING FORKS. By Prof. JOHN S. SHEAVER, Ithaca, N. Y.

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ELASTIC PROPERTIES OF GLASS. By Prof. W. S. FRANKLIN and L. B. SPINNEY, Ames, Iowa.

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A PRELIMINARY STUDY OF THE CONSTANT OF THE MORLEY INTERFERENTIAL COMPARATOR. By Prof. WM. A. ROGERS, Waterville, Me.

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APPLICATION OF INTERFERENTIAL METHODS TO MEASUREMENT OF EXPANSION OF LONG BARS. By Prof. EDWARD S. MORLEY, Cleveland, O., and Prof. WM. S. ROGERS, Waterville, Me.

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SOME RAPID CHANGES OF POTENTIAL STUDIED BY MEANS OF A CURVE-WRITING VOLTMETER. By G. S. MOLER, Cornell University, Ithaca, N. Y.

# SECTION C.

# CHEMISTRY.

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## *TWENTY-FIVE YEAR'S PROGRESS IN ANALYTICAL CHEMISTRY.*

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IN his ninth letter Liebig discusses the influence which glass, cork, platinum and caoutchouc have had upon the progress of chemistry, and shows how difficult it would have been for the science to have developed without them. Not a few great steps forward have hinged upon little things. How much slower, for example, the development of organic chemistry would have been without the potash bulb.

It would be difficult to determine exactly how much influence the Bunsen lamp, the Bunsen pump and the Gooch filter have had upon the evolution of chemistry; that they have greatly facilitated our progress cannot be doubted.

One of the marked changes in analytical methods which lapse of years has brought about is in the time necessary for their performance. In 1868 analyses were made, almost without exception, by persons usually employed in teaching. Only here and there in the larger cities an adventurous pioneer, depending altogether for support upon fees received for doing analytical work had established himself. Nowadays careful analysis is the foundation stone of nearly all our larger industries and the number of determinations made has increased a million fold.

It has become important, therefore, that the work shall be done rapidly, and this necessity has led to careful revision of the meth-

ods used; and to a great variety of devices for obtaining results speedily with the least possible sacrifice of accuracy.

Twenty-five years ago, to determine phosphorus in steel required three days, or two at least. It is now commonly made in half an hour and sometimes in twelve minutes. The process in essentials is, however, almost the same, the details only have been varied. In 1868 silicon was determined in pig iron by dissolving in hydrochloric acid, evaporating to hard dryness, redissolving, filtering, drying, igniting, fusing with sodium carbonate, dissolving, evaporating with hydrochloric acid, dissolving, filtering, drying, igniting and weighing; the whole operation taking nearly a day. The determination is now made in fifteen or twenty minutes. At South Bethlehem, for example, a sample of pig iron to be used is taken as the molten metal runs into the ladle by running it into water, the granules obtained dry spontaneously, are pulverized by a few blows of the hammer, weighed, dissolved, evaporated, filtered, ignited and weighed, while the ladle full of metal is being drawn to the converter. The result of the analysis is sent to the blower by an annunciatior, while the metal is still on its way, and when it arrives he is in full possession of the necessary information for treating it intelligently.

In 1868 the number of persons pursuing analytical chemistry as a business was very small. In the Lehigh Valley, Pennsylvania, a district which took the lead in the iron industry, there were not at that time more than two or three persons capable of making an analysis. Now the number of persons who would come under the same head, most of them actually engaged in such work, is about fifty. This increase is only typical of what has taken place elsewhere. A very large number of furnace managers, steel-works and other superintendents have been recruited from the ranks of the analytical chemists, so that the occupation is now highly esteemed as one requiring men of a high order of ability and attainment.

The analytical chemist of to-day is not content with an answer to the question, how much of each constituent does the substance contain? He must also be prepared to say how the constituents are combined. In other words there is a constant effort for proximate analysis. This requires a far wider knowledge. The successful analyst must, therefore, be a man of truly scientific spirit to successfully attack the various problems that come to him.

In chapter I of his "Metallurgy of Steel" Howe says, speaking of the probable occurrence of minerals in steel. "If these views be correct then, no matter how accurate and extended our knowledge of ultimate composition, and how vast the statistics on which our inferences are based, if we attempt to predict mechanical properties from them accurately we become metallurgical Wigginses. For while we may predict that silicious rocks will usually be vitreous, July hot, April rainy, and phosphorous steel brittle, yet when we go farther and predict accurately, we state what is not inferable from our premises; it may and sometimes does snow in July; Christmas may be warmer than Easter; the more silicious may be less vitreous than the less silicious rock; and more phosphoric steel tougher than the less phosphoric one. In vain do we flounder in the sloughs and quagmires at the foot of the rugged mountain of knowledge seeking a royal road to its summit. If we are to climb, it must be by the precipitous paths of proximate analysis, and the sooner we are armed and shod for the ascent, the sooner we devise weapons for this arduous task, the better."

In 1879 Thoulet (Bull. de la Soc. Min. de France, 1879, No. **1**) proposed the use of a solution obtained by dissolving mercuric iodid in potassium iodid for the separation of minerals having different specific gravities. Church (Min. Mag., Nov., 1877) had proposed a similar method nearly two years earlier. R. Breon (C. R., **90**, 626) proposes the use of a mixture of fused chlorids of lead and zinc, the specific gravity ranging between two and four-tenths and five. D. Klein (C. R. **93**, 318) uses cadmium borotungstate and C. Rohrbach (Ann. Phys. in Chem. (N. F.) **20**, 169) a concentrated solution of barium mercuric iodid. The use of the electro-magnet for this sort of separation of course suggested itself long before this. In 1887 Mackintosh proposed (J. Anal. Chem., **1**, 10) hydrofluoric acid. Shimer has applied the gravity method to the separation of the minerals contained in cast iron, using for the purpose an inclined plane of glass, and Clarke and his assistants have examined the action of hydrochloric acid. A great deal of microscopical work has also been done in the identification of minerals contained in rock. As the result of this and other work the way seems to be opening for a satisfactory proximate rock analysis. In the analysis of the metals much less progress has been made. Shimer finds titianium carbide, TiC (J. Anal. Chem., **1**, **1**) in pig iron, and he and Schneider have each discovered a spontaneously

inflammable iron phosphid in the same material. Abel (Iron, 1883, 1, p. 76 and 1885, 1, p. 115) obtained a carbide of iron, FeC, from steel. In other fields a great deal of the same sort of work has been accomplished but still more remains to be done. How much do we know, for example, of the constitution of coal or of the coal tar made from it? Almost nothing, although they are two of our great staple commodities.

The progress in metallurgical development within the past twenty-five years has been phenomenal. Railroads, telegraphs, electric light and power, agricultural machinery and other factors have in each case created new demands for iron, steel, copper and its alloys, lead, zinc, tin and most recent of all for nickel and aluminum. Each of these metals has a literature of its own and a special development in analytical chemistry. The development of the analytical work has gone hand in hand with the metallurgical treatment. In this work, as in much that has been done in other fields, the necessity of rapid methods has led to the abandonment of many older methods accurate enough but consuming too much time. The analyst nowadays seeks to develop those methods which allow him to determine in a single sample one constituent rapidly and accurately. It is much quicker and usually more accurate to determine phosphorus in one sample and sulfur in another than to separate phosphorus from sulfur and then determine each separately in the same sample.

The time at my disposal will not allow of an account of the advance in all lines of work, not even of a complete account of the general methods which have been used. I must, therefore, of necessity limit very much the scope of my remarks.

In chemical apparatus, twenty-five years have witnessed great improvements. A larger market has encouraged manufacturers of apparatus to study their product more carefully and the result is seen in better shapes, better material and, in graduated material, better graduation. The amount of poor apparatus still turned out is, however, far too large. The balance has been greatly improved. The short arm balance to allow of more rapid work has been introduced. Agate knife edges to resist corrosion, aluminum beams to give lightness, better pan and beam arrests, and better weights are among the changes introduced in the older form. Thanks to an American chemist, Dr. Alfred Springer of Cincinnati, we have now at our disposal a balance made upon an entirely dif-

ferent principle, the so-called "Torsion Balance," which with even greater sensitiveness carries a much heavier load.

Improvements have been made in the Bunsen lamp adapting it to special purposes; one of the most useful of these special forms is Dr. Gibbs' ring burner, allowing the heating of the upper part of crucibles and evaporation of high boiling liquids without danger of loss from spattering.

In the development of other special forms T. Fletcher and Robert Muencke have taken the lead. For ordinary use Morton's burner still retains its popularity and can now be obtained for about one-third of its cost when first invented in 1875.

Gasoline has been almost the only source of heat for the large number of chemists who find themselves remote from gas supply; and burners have been especially constructed for use with this liquid.

Bunsen's famous paper "Ueber die Auswaschen der Niederschläge," in which he describes the filter pump, was published in Liebig's Annalen for Dec., 1868, just within the quarter century period. This pump and the nearly related jet pressure pump have been introduced nearly everywhere, and naturally have had many modifications. An account of these and a comparison of results obtained from the different forms may be found in a paper by T. Fairley (J. Soc. Chem. Ind., **6**, 65).

For some filtrations the paper filter has always been unsatisfactory and a great many devices have been used in the effort to supplant it in whole or in part, such as Gibbs' sand-filter, Munroe's clay-filter and Carmichael's siphon-filter. So far as I am aware, the crucible with perforated bottom was first used in the laboratory of Lafayette College in 1875 or 76 and was devised by Dr. T. M. Drown at that time professor of chemistry. Unfortunately, his experiments were confined to an effort to filter through a paper disk, but the paper would clog and the apparatus was finally laid aside only to be rediscovered and successfully operated by Gooch in 1879 by the use of an asbestos felt. This piece of apparatus is certainly of the greatest value and allows us to make many determinations easily and rapidly that before were difficult or impracticable.

A valuable improvement in filter paper had been introduced by P. T. Austen (Chem. News, **37**, 149). This is the extraction with hydrofluoric and hydrochloric acids, leaving what is practically an ashless paper. I have used papers 11 centimetus in diameter which

after this treatment gave an ash weighing but .00003 gram, and filters of this size are now to be had as a regular article of commerce, the ash of which never exceeds .00008 gram and is frequently less. For less exact work where rapid filtration is desired a corrugated paper has recently been introduced which seems likely to be of considerable value.

A great many reagents which were expensive twenty-five years ago have now become ordinary commercial articles and find wide use. Among these are hydrogen dioxid, bromin, and potassium permanganate. Wax vessels have been devised for cheaply keeping and transporting hydrofluoric acid. Two new metals have been added to the list of those of which laboratory utensils can be constructed, nickel and aluminum.

No satisfactory substitute for platinum has yet been discovered. The demand for this metal has increased greatly with the spread of the incandescent electric light, with a considerable increase in price, and platinum vessels now form a large item in the cost of equipping a laboratory.

The most important constants for chemists are the atomic weights; and no other determination requires such careful preliminary drill, painstaking care, and absolute self-devotion. Unfortunately, a great deal, even of such work, has been rendered useless by the increase of our knowledge or by oversights which were certainly forgivable. A great many valuable atomic weight determinations have been carefully collated and sifted by Prof. F. W. Clarke, now universally acknowledged to be the best living authority upon this subject. As good illustrations of the best work in this field, we have the recent work of Morley upon oxygen and of Richards upon copper.

It is out of the question to give even an outline of the work that has been done in the last quarter century in gravimetric analysis. A mere enumeration of the names of those who have added to our knowledge would cover many pages. To illustrate the progress made I think I can do no better than to take up the single and, economically, the most important metal, iron, and give a brief outline of the increase of our knowledge in this field. The substances of special importance in iron and steel are phosphorus, carbon, sulfur, silicon, manganese and, of late, nickel. The methods for the determination of phosphorus which with the history of their development have been discussed in an admirable paper by A. A.

Blair (J. Anal. Chem., **2**, 97) are both of them more than twenty-five years old, one, the Sonnenschein or molybdate method, having been first used in 1850 and the other, the acetate method, in 1846. The details of both methods have, however, been subjected to minute and careful scrutiny by a very large number of careful and skillful workers and brought to great perfection both in point of accuracy and rapidity of execution.

Carbon, as we know, exists in iron in two, perhaps in three, forms but for the analyst, as yet, but two forms exist. Ullgren's method for total carbon was introduced in 1862 (Ann. Chem. Pharm., **124**, 59). As first proposed, this has been found to be inexact, some of the carbon being oxidized to CO instead of $CO_2$.

The method now almost always used is some modification of the Pearse–McCreath method proposed in 1877. For very rapid work the comparative method of Eggertz, introduced in 1863, has been modified only in a few details. In 1874 Drown showed that nitric acid was a much better solvent for determination of graphite than hydrochloric acid which had before that been used almost entirely. Very little real progress has been made in determining sulfur. It has been found that the sulfur given off as hydrogen sulfid by solution in acid may be oxidized to sulfuric acid, but very often part of the sulfur remains undissolved in unknown combinations and some of it, perhaps, in certain cases, passes off in a form other than hydrogen sulfid which escapes oxidation, so that the tendency is to return to the older oxidation method. For rapid work the hydrogen sulfid is very commonly absorbed by a cadmium solution (Morrell) and afterwards titrated; or it is absorbed in alkali and determined with iodin solution.

In silicon determination some modification of the Drown–Shimer method is nearly always used. This method was I think of Swedish origin, but not until nitric acid had been added to the sulfuric, at first used alone, did the method become accurate and reliable. The determination is of course a very common one and the new method being both rapid and accurate is of very great value. Many new methods have been proposed for the determination of manganese among them the methods of Ford (Trans. A. I. M. E., **9**, 397), Volhard (Am. Chem., **1**, 98, 318), Williams (Trans. A. I. M. E., **10**, 100), Deshay (Bull. Soc. Chem., June 20. 1878), Pattinson (J. Chem. Soc., 1879, 365) and Peters (Chem. News, 33, 35).

The method devised for obtaining accurate samples of cast iron.

borings by Shimer in 1886 (Trans. A. I. M. E., **14**, 760) has proved to be absolutely essential to accura'e work.

Methods for determining iron, of use mainly for iron ores, have been greatly improved, chiefly in the perfection of rapid methods for the reduction of iron to ferrous condition to be afterwards determined either by Marguerite's or Penny's method. Jones' reductor, a long tube containing powdered zinc through which the ferric solution is sucked, seems to be the most generally approved, while Kessler's method which consists in adding stannous chlorid in excess and mercuric chlorid has recently been improved by Mahon. One valuable piece of apparatus, invented for use in this line of work but useful in other work as well, should have mention here. I refer to Ford's filtering and stirring apparatus by which one man may carry on a dozen or more filtrations at one time, or may stir an equal number of solutions.

It is neither necessary nor desirable that I should go on and detail in this way the new methods that have been proposed and used for the determination of each separate metal and non-metal and the methods which have been devised for separating them.

What has been said for iron holds good for the rest, and for nearly all, good methods, many of them excellent, are now known.

Gibbs' method for the electrolytic determination of copper was the first of a whole series of methods worked out on similar lines for this and many other metals, all of modern development. Perhaps the most successful workers in this field have been Classen and Smith. The value of the electrolytic method needs no argument. Best of all, work can often go on while the chemist sleeps, and work started in the evening gives an accurate result the next morning.

The first edition of Mohr's book on "Volumetric Analysis" was written in 1885. He first introduced into practice Griffin's idea of equivalent or normal solutions which since then Wollny (Ztschr. anal. Chem., **24**, 402) and others have proposed in modified form for ordinary reagent solutions. Winckler proposed, in 1883, to double the strength of solutions in order to avoid half molecular weights. In 1888, however, he abandoned this idea, and in volumetric analysis at least the old equivalent weight reigns supreme. In analytical operations everything is sacrificed to utility and nowadays empirical solutions are probably much more common than normal ones. Still more common, perhaps, are approximately nor-

mal solutions of substances which change in strength slightly and a correction for which can be applied by means of a proper table constructed once for all, such as that proposed by Landis (J. Anal. Appl. Chem., **6**, 299). Very few considerable improvements have been made in volumetric apparatus, although Sire's overflow pipette is worthy of notice (Ann. de Chemie et de Physique, 4 ser., **28**, 108). A very large number of new indicators have been used, chief among them perhaps being phenolphtalein and methyl orange. A very careful examination has been made of the behavior of these indicators with different acids at different temperatures and in presence of different salts by R. T. Thomson (C. N., **47**, 123, 185; **49**, 32, 119; J. S. C. I., **6**, 195) and others. This work has aided greatly in their intelligent use. Of the new substances which have been proposed as standardizing materials for alkaline normal liquids, potassium tetroxalate (Ztschr. anal. Chem., **26**, 350) and copper sulfate (J. Anal. Chem., **4**, 424) are perhaps of greatest interest. Among the general methods which have been added to this field are the series of determinations founded upon Volhard's process for silver and thiocyanic acid (Ann. d. Chem., **190**, 1). Bohlig's method founded upon the reaction between barium carbonate and the alkaline sulfates (Ztschr. anal. Chem., **9**, 310), and Quantin's method for the determination of the sulfates founded upon the precipitation of barium from an acid solution of the chromate (C. R., **103**, 102). Nitrogen whenever possible is now measured volumetrically by some modification of Kjeldahl's method (Ztschr. anal. Chem., **22**, 366) which has been of the very greatest practical value. The Reichert, Koettstorfer, Hubl, and other methods used in the analysis of fats also belong here. The colorimetric method has been largely used, especially in water analysis.

In gas analysis a vast amount of work has been done and innumerable pieces of apparatus have been devised for facilitating rapid work. Among the many indefatigable workers in this branch the names of Winkler and Hempel are especially worthy of notice. Lunge's gasvolumeter is also worthy of special mention. Quantitative spectrum analysis belongs almost entirely to the last quarter century. This method seems first to have been applied to the examination of colors by Schiff in 1863. Its use has been greatly extended by Vierordt, Landolt, Vogel, Wolff and others.

I have thus sketched briefly and very imperfectly some of the

more important work that has been done recently in analytical chemistry. In this Columbian year it is not improper I think to remember that a very considerable share of this work has been done by American chemists and to rejoice in it. The published work of Gibbs, Genth, J. L. Smith, Cooke, Gooch, S. W. Johnson, Mallett, Prescott, Caldwell, Bolton, Wiley. Morley, Wormley, Clarke, Chatard, Blair, Dudley, Brenneman, Stone, Carmichael, Munroe, Drown, Morse, Mixter, Richards, Babcock, Ford, Shimer, and of others like them needs no praise of mine. It is an enduring monument to American skill and energy. But I do not wish to claim more than we deserve. It is not to be denied that there is a great and increasing tendency to real scientific work among American analytical chemists, neither can it be denied that we have occasional examples of a spirit of commercialism which shows itself among other ways in advertising certificates, telling part of the truth at least in respect of this or that soap, baking powder, infant food, or disinfectant.

Such things are inconsistent with a high standard of professional honor and should be discouraged in every possible way. They are unworthy of seekers after truth. No scientific man should, I think, put himself in the situation of a lawyer where he may be forced to conceal part of the truth. He is not an advocate but a judge, and should tell the whole truth, and that only.

The profession of the chemist is what we make it and our aim should be a high one, avoiding on the one hand the foolishness which teaches that things that have practical, commercial value are unfit employment for scientific men, and on the other the idea that the best chemist is the expert whose testimony throws most discredit upon the work of his fellow chemist on the other side.

I cannot close without a word upon a phase of the subject not generally taken into consideration by chemists themselves. I refer to the aid they have given in solving certain problems which concern the whole race. How far has the analytical chemist helped in cheapening the ordinary necessities of human existence? I think he has done a great deal. Perhaps I can best illustrate by an example in one line of work, although the same thing would hold good in other lines to a greater or less degree. The cost of Bessemer steel twenty-five years ago was far in excess of its price today. If I recollect rightly, this was at one time as high as $140 per ton. It is now not far from $30. Now I do not mean to claim

that this reduction in price and its far-reaching consequences—the reduction in cost of the necessaries of life through cheap transportation, the decrease in cost of so many articles of prime necessity, implements of all kinds, wire fencing, nails, screws and the thousand and one things essential to comfort, and the increased facility in intercourse—not a small factor in the recent progress of the race, are directly attributable to the work of the analytical chemist; still I do claim that his work has been an essential element in it, and that the contribution has not been a small one. To illustrate: How would it be possible to make steel so cheaply if it were necessary to remelt all the iron as it came from the furnace before blowing it? Plainly it would be impossible. And yet it is plain that this would have to be done if it were not possible through the work of Drown, Shimer, Ford and others to determine silicon in fifteen minutes. These men and others like them in this and other fields of effort have contributed in no small degree to our comfort and happiness and though their only reward in the annals of the race may parallel that of the private soldier, who was shot in the leg and had his name misspelled in the morrow's paper, we should not forget them, nor, because they cannot devote their lives to a study of the behavior of orthochlorparadipropylbenzene with sulfonitroprotocatechuic acid, therefore hold their work worthy of contempt.

# PAPERS READ.

THE ACETYL AND BENZOYL DERIVATIVES OF THE PENTOSES. By Prof. W. E. STONE, Purdue University, Lafayette, Ind.

[ABSTRACT.]

BY the action of acetic anhydride upon arabinose or xylose in the presence of anhydrous sodium acetate at a temperature of 100° C. for one hour, new compounds are formed of the composition $C_5H_6O_5\ (C_2H_3O)_4$, in which four acetyl radicles have been introduced into the pentose molecule. This indicates the presence of four hydroxyl groups in the pentose and is in accordance with the accepted constitution of the same.

Tetra-acetyl arabinose is an uncrystallizable oil of pale yellowish color, insoluble in water, soluble in strong alcohol in which solution it is optically active, showing the specific rotation $(\alpha)_D = +26.39°$.

Tetra-acetyl xylose, when first prepared, is an oily substance, but on cooling slowly crystallizes in fine needles. Melting point 123.5° to 124.5°. Insoluble in water, soluble in alcohol in which solution it is optically active showing a specific rotation $(\alpha)_D = -25.43°$.

Benzoyl chlorid acting upon solutions of the pentoses in dilute sodium hydrate (10 per cent) forms new compounds of apparently variable composition. They are insoluble in water; soluble in boiling absolute alcohol from which they separate on cooling. The arabinose compound was oily or waxy; the xylose crystallinic, but neither had a constant composition.

THE ELECTROLYTIC OXIDATION OF GLYCEROL. By Prof. W. E. STONE and H. N. MCCOY, Purdue Unviersity, Lafayette, Ind.

[ABSTRACT.]

FORMIC, acetic, oxalic and glyceric acids, acrolein and trioxymethylen have been recognized as products of electrolytic action upon dilute solutions of glycerol. We have sought for glyceric aldehyde under the same conditions.

Currents of less than .3 ampere were employed; 1° upon dilute glycerol acidulated with sulfuric acid; 2° upon dilute glycerol to which had been added sodium hydrate; and 3° upon dilute glycerol containing a small amount of sodium nitrate.

Under all of these conditions the ultimate result of the electrolytic action was to produce a large amount of acids the nature of which was not closely investigated. In acid solutions acrolein was always produced. In alkaline solution a yellow color was soon produced which persisted as long as free alkali was present, but disappeared as soon as the alkali had been neutralized by the acids formed. In any case the solutions reduced Fehling's solution very strongly *in the cold*, which is characteristic of glyceric aldehyde.

Glyceric aldehyde has not been obtained free, but Fischer has shown that it is polymerized by dilute alkalies to glycerose isomeric with glucose. A weak current was allowed to act on an alkaline, cooled solution of glycerol several days. It became yellow in color and reduced Fehling's solution in the cold. It was removed from the current, enough alkali added to equal 2 per cent and left in the cold four days. It now no longer reduced in the cold but strongly on boiling with Fehling's solution. From it was prepared a phenylhydrazin compound melting at 200° and resembling in a general way the glucose compound melting at 204°. A portion was subjected to the action of yeast and fermented freely. These results indicate the formation of glycerose from the electrolyzed alkaline solution of glycerol.

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## ON THE NITRITES OF SOME AMINES. By Dr. W. A. NOYES, Rose Polytechnic Institute, Terre Haute, Ind.

[ABSTRACT.]

THE nitrites of a number of amines have been studied in order to determine whether the stability of the nitrites of the "alicyclic" amines of Bamberger is due to the ring structure or to some other cause. The work done establishes pretty conclusively that amines of the general structure $\frac{R}{R}CHNH_2$ form nitrites which are stable at ordinary temperatures. The new amines prepared have been di-propyl-carbinamine (4-amino-heptane), and di-iso-butyl-carbinamine (2.6 di-methyl-4-amino-heptane). Di-propyl-carbinamine boils at 139°–140°, has a specific gravity $\left(\frac{20^\circ}{4^\circ}\right)$ of 0.7667 and forms a chloride which is easily soluble in water and alcohol and which melts at 241°–242°. Di-iso-butyl-carbinamine boils at 166°–167°, has a specific gravity of 0.772 and forms a chloride which is moderately soluble in water and which melts at 247°–248°. When the chloride of either base is heated to the boiling point for half an hour with a little more than the theoretical amount of sodium nitrite, about 80 per cent of the calculated amount of nitrogen is evolved. The nitrite of each base is perfectly stable at ordinary temperatures.

The chloride of hexametlylene-diamine[1] (1.4 di-amino R-hexane) when

[1] Baeyer and Noyes, Ber. Deut. Ch. Ges., 22, 2172.

heated with sodium nitrite in the water bath evolves about 23 per cent of the theoretical amount of nitrogen. It neither shows the perfect stability expected of an "alicyclic" amine nor does it decompose in the normal manner. Di-hydro-meso-anthracene[1] yields anthramine when its chloride is boiled with sodium nitrite. Di-phenyl-carbinamine[2] forms a well-crystallized, difficulty soluble nitrite which yields di-phenyl-carbinol when boiled with water. Semmler[3] has shown that 1.3 methyl-amino-R-pentane reacts normally toward nitrous acid and Wislecenus and König[4] have shown the same for amino-hydrindene, $C_6H_4\begin{array}{c}CH_2\\ \\ CHNH_2\end{array}CH_2$. On the other hand, Bamberger[5] has shown that 1.2 phenyl-animo-ethane forms a nitrite which can be crystallized from boiling water. From these facts it seems probable that the strongly basic character of the "alicyclic" amines and the stability of their nitrites is not due to their ring structure but rather to the accumulation of hydrogen in parts of the molecules which are in close proximity to the amine group.

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## Solubility of lead oxide in the normal tartrates and other normal organic salts. With observations on the rotary power of the solutions thus obtained. By Louis Kahlenberg and Dr. Homer W. Hillyer, University of Wisconsin, Madison Wis.

[ABSTRACT.]

Boiling solution of Rochelle salt dissolves litharge with readiness.

Other normal tartrates, viz.:—The normal tartrates of potassium, sodium, lithium and rubidium act similarly.

The solutions are strongly alkaline to test paper, have the odor and feeling between the fingers of caustic alkali and, in fact, alkali can be isolated from them by extracting the residue of evaporation with alcohol. The potassium compound was most thoroughly studied. In this case the alkalinity, as indicated by titration, using phenol-phthalizn as indicator, amounts to the equivalent of one-half the lead oxide dissolved. The alkali set free is more than the amount necessary to hold the lead in solution. On neutralizing with an acid about two-thirds of the acid necessary to neutralize the free alkali may be added before a permanent precipitate forms.

One molecular weight of lead oxide readily dissolves in one molecular weight of the normal tartrate. By persistent boiling a larger amount

[1] Goldmann, *Ibid.*, 23, 2522.
[2] Goldschmidt, *Ibid.*, 19, 3233.
[3] *Ibid.*, 25, 3519.
[4] Ann. Chem. (Liebig), 275, 350.
[5] *Ibid.*, 257, 19.

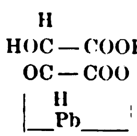
of lead oxide can be dissolved, but the quantity is never more than one and one-fourth molecule to one molecule of tartrate. Solutions, identical in every way, may be made by dissolving lead tartrate in caustic alkali in equivalent quantity. As indicated above, an equivalent amount of alkali is not necessary to give a solution. So here it is found that less than an equivalent of potassium hydroxide will dissolve an equivalent of lead tartrate to yield a solution but slightly alkaline.

From these solutions the lead may be precipitated by hydrogen sulphide or carbon dioxide, leaving a neutral solution of the normal tartrate, thus showing that the lead oxide has not decomposed the tartaric acid.

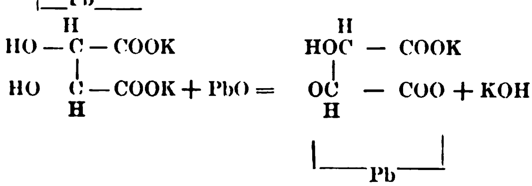
When the solution of lead oxide in potassium tartrate was concentrated, a solid separated on cooling. This, when washed, was found to be a tribasic lead tartrate free from potassium. No other definite compound could be isolated.

Lead oxide is dissolved not at all or only slightly by the normal salts of other organic acids so far as tried, with the exception of the mucates and saccharates. Lead oxide was boiled with solutions of the normal salts of the following acids with negative results: succinic, malonic, malic, citric, acetic, propionic, lactic and glyceric. Potassium mucate and potassium saccharate in very concentrated solutions act on lead oxide, each molecule of the salt changing two molecules of lead oxide into a new compound and partially dissolving it. The solutions are alkaline. From these solutions, on cooling, a tribasic lead salt readily separates.

For the solution of lead oxide by a normal organic salt it then appears necesary that the salt should be derived from a bibasic acid with at least two hydroxyl groups, and in these cases the amount of lead oxide acted on is one molecule for every two hydroxyl groups. In the present stage of knowledge of salts in solution, no hypothesis can be more than a suggestion and not at all an attempt to define structural relations. But as embracing most, if not all the facts, the following is offered as a formula for the substance formed when lead oxide is dissolved in normal potassium tartrate.



and the equation of its formation,



No other oxides were found to dissolve in solutions of potassium tartrate with the exception of arsenious acid and mercuric oxide which dissolved slightly. The oxides tested were $As_2O_3$, $SnO_2$, $Bi_2O_3$, $HgO$, $Ag_2O$

$Pb_3O_4$, $CuO$, $Fe_2O_3$, $Al_2O_3$, $Cr_2O_3$, $MnO_2$, $ZnO$, $MgO$, $BeO$, $Er_2O_3$, $Ce_2O_3$, $Di_2O_3$, $WO_3$.

It was hoped that some light might be thrown on the Fehling solution question, but in this we were disappointed.

Some observations were made of the optical rotatory power of the solutions of compound basic tartrates. It has been found by Oudemans that when an active base is combined with an inactive acid the nature of the acid has no influence on the rotatory power so that the molecular rotation of all salts of active bases is the same. A similar law has been deduced in regard to the salt of active acids and inactive bases. Schneider found that the malates have the same rotatory power and Landolt found that the camphorates and the tartrates have the same rotatory power.

In contrast to this the rotatory power of some of these alkaline tartrates may be of interest. The following shows the rotatory power of equivalent amounts of tartrate made from 1.25 gr. of tartaric acid made up to 200 cc. and viewed in a 200 mm. tube:

| | | | |
|---|---|---|---|
| 1. | $K_2T$ | | 31′ |
| 2. | $Tl_2T$ | | 27′ |
| 3. | $(NH_4)_2T$ | | 30′ |
| 4. | $K_2T$ | + PbO | — 06′ |
| 5. | $K_2T$ | + ZnO | + 14′ |
| 6. | $K_2T$ | + $Tl_2O$ | + 12′ |
| 7. | $(NH_4)_2T$ | + $Ag_2O$ | + 75′ |
| 8. | $(NH_4)_2T$ | + ZnO | + 25′ |

Nos. 5, 7, 8 made by dissolving the tartrates of the heavy metals in excess of the alkali.

When one molecule of lead oxide is dissolved in a solution of one molecule of the normal tartrates, the solutions obtained show the following rotatory powers: In this case the strength of the solution is double that of those in the last table, *i. e.*, 1.252 gr. tartaric is changed to normal tartrate and the solution after dissolving 1.829 grams (1 mol.) of lead oxide is filtered and polarized.

| | $\frac{1}{3}$ | $\frac{1}{4}$ | specific from full [a] |
|---|---|---|---|
| Rb + 9′ | — 10′ | — 8 | D. + 2° 16′ |
| K — 17′ | — 33 | — 12 | — 6° 42′ |
| Na — 30′ | — 33 | — 19 | — 7° 19′ |
| Li — 43 | — 30 | — 21 | — 11° 03′ |
| $NH_4$ — 116 | — 47 | ppt. | — 26° 43′ |

If we consider the column of full strength or of one-fourth strength or the specific rotatory power, we see that with the exception of the ammonia there is a regularity, the rotatory power is less as the atomic weight of the alkali metal is less. The ammonia is an exception, but this solution was made by dissolving lead tartrate in an excess of ammonia and the nitrogen compounds are not unfrequently abnormal.

The same objection to speculation holds here as that before mentioned.

The substances dealt with are salts in solution so that comment on the structure is of very little value. These facts are so far as they go in line with the interesting expansion of Le Bel Van't Hoff's hypothesis formulated by Guye but it is not well to push them too far.

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ON THE ATOMIC WEIGHT OF OXYGEN. By Prof. EDWARD W. MORLEY, Station B, Cleveland, Ohio.

[ABSTRACT.]

THE paper gives a resumé of the work hitherto accomplished by the author in his work on the atomic weight of oxygen, and mentions results obtained by the following processes:

1. Synthesis of water from weighed quantities of hydrogen and of oxygen.
2. Determination of relative densities of the gases, the measurement of pressures and volumes being made at ordinary temperatures.
3. Determination of absolute density of oxygen by measurements at the melting point of ice.
4. Determination of absolute density of hydrogen by measurements at the same temperature.
5. Preliminary work on a new determination of absolute density of hydrogen by measurements at the same temperature.
6. On the ratio of the volumes in which the two gases combine.

[This paper will be printed in Smithsonian Contributions to Knowledge.]

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THE CONSTITUTION OF PARALDEHYDE AND METALDEHYDE, By W. R. ORNDORFF and JOHN WHITE, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

DETAILS are given of experimental data establishing the molecular weight of metaldehyde. These are followed by theoretical considerations on the constitution of the two polymeric modifications of aldehyde, leading to the following conclusions:

1. Paraldehyde and metaldehyde have the same percentage composition, the same molecular weight, and the same plane structural formula.
2. From their general chemical conduct, they must be regarded as stereo or geometrical isomers.
3. Paraldehyde being the more stable, probably is the *cis. trans.* modification while metaldehyde is the *cis.* form.

[See American Chemical Jonrnal, Vol. 16, p. 43, for the complete article.]

THE ACTION OF GASEOUS HYDROCHLORIC ACID AND OXYGEN ON THE PLATINUM METALS. By Prof. WM. L. DUDLEY, Vanderbilt University, Nashville, Tenn.

[ABSTRACT.]

A MIXTURE of gaseous hydrochloric acid and oxygen or air was passed over platinum, iridium, osmium, rhodium, ruthenium and palladium, each in a finely divided state. The metals absorbing these gases caused the decomposition of the HCl and the nascent chlorin and chlorides of the metals were formed.

It was also found that the finely divided metals moistened with HCl were attacked on exposure to air or oxygen.

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THE ELECTRO-DEPOSITION OF IRIDIUM: A METHOD OF MAINTAINING THE UNIFORM COMPOSITION OF AN ELECTROPLATING BATH WITHOUT THE USE OF AN ANODE. By Prof. WM. L. DUDLEY, Vanderbilt University, Nashville, Tenn.

[ABSTRACT.]

A DESCRIPTION of a method devised in 1884, solution of sodium iridichlorid, ammonium iridichlorid, and ammonium iridisulfate were found to give good results. The bath was kept of uniform strength and composition by using anodes of carbon surrounded by linen bags containing iridium hydrate which dissolved as fast as the acid was set free. This is a method applicable generally where the metal to be plated, used as an anode, is insoluble.

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ON THE OCCURRENCE AND DISTRIBUTION OF NITROGEN IN DEEP ARTESIAN WELLS OF THE MISSISSIPPI BASIN. By Prof. E. G. SMITH, Beloit, Wis.

[ABSTRACT.]

THE paper discusses the phenomenal amount of ammonia found in some deep artesian wells (with table of analyses) and offers an explanation of same as a result of reduction of nitrites and nitrates due to hydrogen sulfid.

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THE ADVANTAGES OF EXTENDED EXAMINATIONS OF RIVER WATERS. By Prof. E. H. S. BAILEY and E. C. FRANKLIN, Lawrence, Kas.

[ABSTRACT.]

IN many investigations on water supply the local conditions only are taken into account. The authors have had an opportunity to examine, to

a limited extent, the waters of the Kansas river and of the streams composing it. They have found that the waters are widely different in composition, regarded from an economic or a sanitary standpoint. The methods both of Frankland and Waukiyn have been followed, and the results point generally to the same conclusions. The source of the ammonia is discussed and the cause of an increase is investigated. The unreliability of any conclusions, based upon the chlorine determinations for such waters, is pointed out. Finally, the importance of ammonia standards for different localities is suggested and illustrated.

[This entire paper will be published in the Kansas University Quarterly, 1894.]

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SOME EXPERIMENTS ON SAMPLING BY QUARTATION. By P. W. SHIMER and S. K. RIEFSNYDER, Easton, Pa.

[ABSTRACT.]

EXPERIMENTS to determine the best method of obtaining an accurate sample from a large amount of heterogeneous material. The best results were obtained by wetting the coarse powder, mixing thoroughly, flattening the pile by pressure under a board and quartating in the usual way.

[Will be printed in the Journal of the American Chemical Society.]

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THE ANALYSIS OF LUBRICATING OILS CONTAINING "BLOWN" RAPE-SEED AND "BLOWN" COTTON-SEED OILS. By THOMAS B. STILLMAN, Stevens Institute, Hoboken, N. Y.

[ABSTRACT.]

THIS paper shows how the proportion of "blown" cotton and rape-seed oils may be determined in commercial paraffin lubricating oils and shows that the chemist's analysis is not complete unless his report includes the method of duplicating the sample submitted.

[Will be printed in the Journal of the Chemical Society.]

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NATURAL GAS FROM NEW LISBON, OHIO. By Prof. W. A. NOYES, Terre Haute, Ind.

[ABSTRACT.]

THE specimens of gas examined were found to have the following composition:

| | | |
|---|---|---|
| Methane, | 67.0 | per cent. |
| Ethane, | 11.1 | " " |
| Carbon dioxide, | 1.2 | " " |
| Oxygen, | 0.9 | " " |
| Nitrogen, | 19.8 | " " |
| | 100. | |

The determination of methane and ethane was based on the volume of carbon dioxide formed, and the contraction experienced, when the gas was exploded with an excess of oxygen. A direct determination of nitrogen, by burning the gas with copper oxide, gave 22.7 per cent. This indicates that the gas may have contained some propane or other higher homologues of the marsh gas series. The oxygen was probably not originally present in the gas.

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ON THE SYSTEMATIC ERRORS AFFECTING ALL THE ATOMIC WEIGHTS OF STAS. By Dr. GUSTAVUS HINRICHS, St. Louis, Mo.

[ABSTRACT.]

THIS paper is limited to the consideration of only *one* each, mathematical and chemical error — systematic, in character affecting all weights alike, not merely one.

*Mathematical.* Overlooking the small unavoidable errors of material and operation, the identical symbols used in the formulæ do not represent identical weights in fact. Hence, elimination leads necessarily to false results; extent of error committed fully 0.0006 per unit.

*Chemical error.* Atomic weight in its very nature is a constant. But Stas determinations themselves show the atomic weight of each element dependent on the total amount of that element used in the determination. Hence, a systematic chemical error was committed.

Either of these two errors would invalidate all the numerical results of Stas. Hence, not one of the atomic weights of *Stas* is of any value in chemistry of precision.

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AN UNUSUAL FORM ON CALCIUM GLYCERATE. By Prof. LAUNCKLOT ANDREWS, Iowa City, Iowa.

[ABSTRACT.]

THE calcium glycerate was a specimen prepared in the usual way by the action of nitric acid upon the purest crystallizable glycerine. The micaceous plates, of which the preparation consisted, contained five molecules of water of crystallization instead of two as usual. Besides determinations of water and of calcium, a complete combustion was made. The results confirmed the formula $Ca\,(C_3H_5O_4)_2 + 5H_2O$ and demonstrated the purity of the preparation. The conditions under which this salt crystallizes with five molecules of water are obscure and the author, despite every effort, has not succeeded in duplicating the unique specimen referred to.

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NARCEINE, A NEW FORMULA AND NEW DERIVATIVES. By Dr. G. B. FRANKFORTER, State University, Lincoln, Neb.

THE STRUCTURAL FORMULA FOR NARCEINE AND ITS SYNTHESIS FROM NARCOTINE,—PSEUDO-NARCEINE. By G. B. FRANKFORTER, State University, Lincoln, Neb.

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A TEMPERED STEEL METEORITE. By Dr. E. GOLDSMITH, Philadelphia, Pa. [Printed in Proc. Acad. Nat. Sci. Phil.]

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ON "A REVIEW OF ATOMECHANICS." By Dr. GUSTAVUS HINRICHS, St. Louis, Mo.

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THE ACTION OF SODIUM ON ACETON. By Prof. P. C. FREER, Ann Arbor, Mich. [Printed in Amer. Chem. Journal.]

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## ELEVENTH ANNUAL REPORT OF THE COMMITTEE ON INDEXING CHEMICAL LITERATURE.

THE Committee on Indexing Chemical Literature presents to the Chemical Section its eleventh annual report.

During the past year the following bibliographies have been published:

1. Index to the Literature of Stereochemistry. By Arnold Eiloart. J. Amer. Chem. Soc., XIV, pp. 241–284.

This carefully prepared index also accompanies the author's Guide to Stereochemistry issued independently (New York, 1893). The material is arranged alphabetically by authors, with a chronological classification of publications and a special list of reviews.

2. Index to the Literature of Explosives. Part 2. By Charles E. Munroe. Baltimore, 1893. pp. 43–195. 8vo.

This completes the undertaking begun several years ago by Professor Munroe; Part 1 was issued in 1886; twelve sets of periodicals embracing 984 volumes have been carefully examined.

3. Materials for a Monograph on Inuline. By J. Christian Bay. Trans. Acad. Sci., St. Louis, VI, 151 (1893). 9 pp. 8vo.

4. Bibliography of the Tannoids. By J. Christiana Bay. Fifth Ann. Report Missouri Botanical Garden (May, 1893) 27 pp. 8vo.

5. Digest of the Studies on the Nature of the Organic Non-sugar in Saccharine Products. By J. A. Deghuée. School Mines Q., XIII, pp. 313–334. (1893.)

A subject-index followed by an author and title bibliography.

6. Bibliography of Mineral Waters chronologically arranged. By Paul Schweitzer. Appendix C, Report on the Mineral Waters of Missouri, Vol. III, Geological Survey of Missouri, December, 1892. pp. 237–244.

This is chiefly a translation of Osann's Darstellung der bekannten Heilquellen, Berlin, 1829–43, with additions by Professor Schweitzer.

7. A Bibliography of Analytical and Applied Chemistry for the year of 1892. By H. Carrington Bolton. J. Anal. Appl. Chem. Vol. 7, p. 19.

8. A Select Bibliography of Chemistry; 1492–1892. By H. Carrington Bolton. Smithsonian Miscellaneous Collections, No. 850. Washington City, 1893. pp. XVI–1212. 8vo.

This work is the most extensive bibliography of chemistry hitherto published, yet it does not claim completeness; it contains 12,031 titles in twenty-four languages divided into seven sections, viz.: I. Bibliography. II. Dictionaries. III. History. IV. Biography. V. Chemistry, pure and applied. VI. Alchemy. VII. Periodicals. Appended to the section of periodicals is a list of Abbreviations of the Titles of Chemical Periodicals, which is an extension of the list printed in the Fifth Annual Report of this Committee. A Subject-Index of thirty pages, double columns, concludes this important work.

Reports of progress have been received from several chemists. Prof. J. Christian Bay of the Missouri Botanical Garden, St. Louis, Mo., whose published works are noticed above, is preparing a bibliography of alcoholic fermentation with special reference to vegetable physiology. He will be very glad to receive notes of the literature on this subject from botanists, chemists and physiologists interested in having the bibliography as complete as possible.

Dr. Alfred Tuckerman expects to get ready for printing, during the coming year, that portion of his Bibliography of Mineral Waters which relates to the United States.

At a formal meeting of this Committee held in Rochester last August, it was voted to accept the following bibliography presented by Prof. A. B. Prescott, and to recommend it for printing to the Smithsonian Institution:

A Bibliography of Aceto-acetic Ester and its Derivations, including an Index of Literature on the subject. Compiled by P. H. Seymour, Assistant in General Chemistry, University of Michigan, June, 1892.

This work is now passing through the press and will form one number of the Smithsonian Miscellaneous Collections.

Prof. H. W. Wiley will present to the Congress of Chemists at the Columbian Exposition the following communication:

A Partial Bibliography of the Occurrence of Copper, Tin, Lead and Zinc in Food and Drink, Vegetable and Animal Tissues, etc. By Mrs. Karl Thayer Pomeroy-McElroy.

This bibliography is styled partial because confined to only fifteen periodicals which, however, are exhaustively indexed. It embraces nearly 1500 titles. The channel of publication is not yet determined.

The Chairman of your Committee will lay before the same Congress of Chemists a statement on bibliography including a plan for an International Index to Chemical Literature; in this the work of this Committee is briefly set forth.

Respectfully submitted,

H. Carrington Bolton, *Chairman*,
F. W. Clarke,
Albert R. Leeds,
Alexis A. Julien,
John W. Langley,
Albert B. Prescott,
Alfred Tuckerman.

SECTION D.

# MECHANICAL SCIENCE

AND

# ENGINEERING.

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F. R. JONES.

ADDRESS

BY

# S. W. ROBINSON,

VICE PRESIDENT, SECTION D.

## *TRAINING IN ENGINEERING SCIENCE.*

THREE facts sufficiently support the proposition that a thorough and systematic training in the sciences that underlie the professions of the engineer is eminently fitting, viz.:

First, the important present and future bearing of engineering practice upon the welfare of the country.

Second, the comprehensiveness of the training, embracing as it does the fundamental principles of all the mechanical arts and manufactures.

Third, the extended influence of a sound engineering practice toward the elevation of the human race, since the progress of any nation is measured almost directly by the extent it favors and utilizes, in the various fields of industry, the same professions, arts and trades which it is the object of this training to enforce.

The teachers of engineering are thus in the highest degree accountable for the proper utilization of the forces and materials of nature's laboratories by a right training and directing of the minds of those who are to lead in the material welfare of nations.

Before the existence of schools of engineering, progress in the corresponding works and industries of the world was comparatively slow. The works requiring an engineer formerly were large buildings and road bridges, mostly of stone, where an abundance of material and extravagance of application under simple rule of thumb took the place of engineering science. Less than one hundred years ago were civil engineering schools established and

but few till within fifty years; twenty-five years ago mechanical, and quite recently electrical. As a rule, demand for these schools precedes them, and the marked impetus they imparted to their respective branches of the world's progress cannot be denied.

Thus, note the remarkable profusion of works in the civil engineering line which has appeared since the inauguration of schools of civil engineering. Our present great iron and steel structures are, and must be, based on scientific principles.

Where one branch of engineering had no training school, best progress was made by those who obtained a preliminary training in the nearest allied school. Thus, architecture and engineering borrowed from mathematical courses, civil engineering from military and architectural, mechanical from civil engineering, electrical from mechanical, mining engineering from chemical and civil engineering courses, etc.

For instance, before mechanical engineering schools were established, progress in mechanical lines of practice was comparatively slow, the most efficient college aid having been obtained from the schools of civil engineering.

Then an accomplished machinist who also was a graduate in civil engineering had such a start in experience and principles as enabled him to comprehend and make fair progress in the application of the special principles of mechanical engineering. In this way the early efficient mechanical engineers were trained.

But this serving of double time for a single object proved to be too slow for the inborn progressive tendencies of the country.

The delay in starting these schools may be explained as due to two causes: first, the inordinate conservatism of the old college schools deprecating the training of the mind in matters touching material things; and second, the fact that prior to that time mechanical affairs included only such as were embraced within the bundle of slow growth, empirical knowledge and judgment, where no very great and sudden departure from existing rules and precedents was found necessary.

There is no doubt that all human progress was hampered by this dearth of knowledge of engineering science, a fact evinced by the absent of bold or startling moves in the material world prior to that time, except where the training from the earliest allied schools aided in the enlargement of construction as in the case of the Fairbairn iron water wheels, the typical marine engine of forty years ago, etc.

Probably no better criterion of this can be found than by referring to the mechanical progress of this country in recent years in connection with the issue of patents, which within the past twenty-five years are seven times as numerous as in the eighty years preceding, a rate of issue during the existence of our mechanical schools nearly twenty-fold greater than before. The study of mechanism in our schools is to be regarded as the great incentive to this profusion of patents, as well as to the actual development of machinery in this age, as the tremendous strides of application in the industries of novelties of invention bear witness.

Mechanical design in construction is also to-day vastly different from that of thirty years ago, the advance being scarcely less marked than that of applied mechanism. Instead of the Grecian architectural elements formerly employed as the *ne plus ultra* of design, we now find forms as if of natural growth, indicative of strength and rigidity, and of superlative beauty from their very fitness, as attested by a glance at the great array of modern machinery, from the machine tool on to include motors, machinery of transmission and for manufacturing.

In the study of structures it is known from the standpoint of science that sudden changes of motion produce destructive shocks; dead resistance in the machine itself, abrasion and wear; and scantiness of section overstrain; wherein are found causes of the three most deadly diseases which mechanical structures are heir to, and the engineer of to-day knows better than those before him how to apply the remedy in advance of the ailment. Thus, successful engineering practice requires a wide range of familiarity with the principles and laws of motive force, dynamics, mechanism, resistance and qualities of materials, electricity, metallurgy, etc., as essential requirements; and these must, for the highest success, be acquired in our schools of engineering. In view of the foregoing considerations, the energetic efforts of our various professors and teachers of engineering to develop their individual schools with scarcely a conference with their fellows, makes reasonable a material diversity in the showing presented. Unification of means and method and the greatest good for all will, therefore, furnish ample ground for frequent conferences of engineering teachers.

Granting that the location of a school will account, in a measure, for the quality of material found in its students, and that the neighboring country will reasonably make special demands upon

it, yet the subjects taught will of necessity be mostly kindred with those of other schools and the instruction will, for highest efficiency, be largely with similar methods and means. What these shall be must be determined as based upon the aggregate of experience and judgment of those engaged as teachers. This is not to say that each teacher is to blindly follow a stereotyped course laid down by a congress, but it evidently does not conflict with the idea of a so-called "standard" course serving as a guide, and to which all may seek to attain or exceed as to breadth and extent.

Probably, more than in other courses of instruction, engineering presents the greatest diversity of important subjects, a fact which is alone sufficient to account for variation in the prescribed work of different schools in some striking particulars; variations sometimes due to the estimated value of particular studies when adopted in the courses and sometimes due to a lamentable extent, to the cost of equipment. One teacher may count instruction in principles and theory as the predominating essential, deferring the knowledge and experience pertaining to the material things to be dealt with in professional practice until acquired in that practice, while others believe that a degree is title-worthy only by those who have both theoretical and practical knowledge. For instance, the power to invent, in the mechanical line, is a most valuable possession, and yet how many of our mechanical schools train the student in this? Without defining, all do it to some extent, but how many when analyzed into three orders where the highest is that of a real novelty, purely unique, the foundation of a new creation such as the original Pitot tube, the Bell telephone, the pneumatic process in foundation, the incandescent lamp, etc.; the second order, that of a new means to a desired end, such as an improved mowing machine, sewing machine, etc.; and the third order, invention, an improvement in form or make-up, such as the bent wood wheel felly, paper car wheel, wire suspension bridge, etc. As an author of an invention of the first order is a real genuis, born and not made, we forbear drilling him in college class exercises, contenting ourselves with calling attention to the fact of such genii. But the second and perhaps the third order inventions may be qualified for on the part of the student, with telling effect by a well chosen line of exercises in connection with class work. Thus, the inventive faculties may be quickened and trained to follow in legitimate channels, whereby a machine for a specific purpose

brought out in professional experience, will be in all its parts well chosen as the result of exhausting the field of invention with respect to every piece. But probably the easiest act of invention is that of giving form to individual pieces or what may more properly be termed "designing." Right here, therefore, is found the most suitable initial point for the class, in a progressive course of instruction in inventing, and whether such a course, brief or extended, should be brought into school work is among the most interesting questions.

Laboratories mean systematic practical training: first, for testing the principles studied; second, for familiarizing the student with the materials and appliances to be met with in his future practice, all planned for securing the greatest possible amount of practical training within the time alloted. Probably none would omit it altogether, nor any adopt only laboratory work in a complete course for a degree. The whole range of laboratory work, now in existence in our various schools, embraces that extending from the mere mechanical operation of the workshop, the chaining of a line or the pulverizing of a mineral, to that of the experimental investigation of the highest laws underlying the profession.

What is the best means for admixture of laboratory and principles is, therefore, another question demanding extended consideration. A most useful result which we may hope to reach is a complete analysis of this laboratory into parts well defined and each estimated in value. Then may be settled the advisability of attempting articles of manufacture and of having such terms as field practice, field work, photograph laboratory, mechanical engineering laboratory, advanced mechanical laboratory, testing laboratory, elementary mechanical laboratory, power laboratory, shop laboratory, school shop, shop work, manual element, etc., pruned down to the advisable necessary number of universally accepted names.

We may also then look for an answer to the paramount questions as to the remaining and higher features of the courses, the studies and the degrees, such as sanitary or hydraulic engineering, railway engineering, stationary steam engineering, electrical engineering, mill and factory engineering, marine engineering, railway machinery engineering, hydraulic motor engineering, etc., and whether a bachelor's degree shall be first conferred at the end of a general course, and after further study or practice, or both, the master's degree, and whether this last should be simply C.E., M.E., E.M., etc., or

this followed by the name of the special branch of engineering pursued. Finally, we must, without doubt, in all these various and important matters as touching our present schools of engineering, concede that there is considerable confusion of ideas, and that a standard course or courses as representing the united judgment of the engineering teachers of the country would be hailed with great satisfaction; not that all will adopt them entirely, but that each may have a standard with which to compare, in order to attain the highest ends in the professions of engineering.

## PAPERS READ.

ECONOMICAL STEAM COMPRESSION. By Prof. J. BURKITT WEBB, Hoboken, N. J.

[ABSTRACT.]

MY attention having been called to some articles on the subject of the best amount of compression in a steam engine cylinder, I have examined them and find that much that has been written is needlessly abstruse.

It should be noted that in none of the discussions, so far as appears, has any allowance been made for loss by cylinder condensation; Mr. Ball's paper, to be read before the American Society of Mechanical Engineers, alludes to it, however, but does not attempt to calculate it.

It is not proposed here to make a detailed review of what has been written and this part of the subject will be passed over with a few remarks.

Professor Cotterill, in "The Steam Engine," gives a discussion of the subject correct for theoretical cards as far as it goes, but treated in a manner calculated to justly intensify the prejudice against mathematical work which exists in the minds of so many engineers.

Professor Eddy's reference, in his "Thermodynamics," to Cotterill's work, fully substantiates the above criticism, for certainly, being correct, it ought, if reasonably simple, to commend itself to one capable of writing a treatise on the subject of heat. The reference is as follows:

"Cotterill has, in his valuable treatise, investigated the problem in this (just explained) manner, but the results, which can differ from those just obtained by only an insignificant amount, are not characterized by simplicity of conception or expression."

It might be supposed from the comparison contained in this reference, that Cotterill and Eddy, working by different methods, arrived at essentially the same result, whereas the different method, pursued by Eddy and others previously, leads to a result, the supposed erroneous nature of which was pointed out by the editor in Engineering, March 5, 1875. The cautious language, used in the comparison, suggests that Professor Eddy simply accepted Cotterill's treatment as correct and, believing his own to be right, ventured the conclusion that they could therefore differ "by only an insignificant amount." The fact is that Cotterill and Eddy worked upon two different problems.

Having been asked whether, to obtain the most economical compression, the real ratio of the compression should be made equal to the real ratio of

expansion, as stated by Eddy and others, it was thought easier and safer to work the problem out than to fully examine what has been written on the subject. After making the solution given below, the result was found to agree with that given by Cotterill.

*Problem.* Let $A\ B\ C\ D$ be an indicator card without compression and let it be required to cut off the corner $D$ by compression so as to effect the greatest saving of steam, the expansion remaining unchanged.

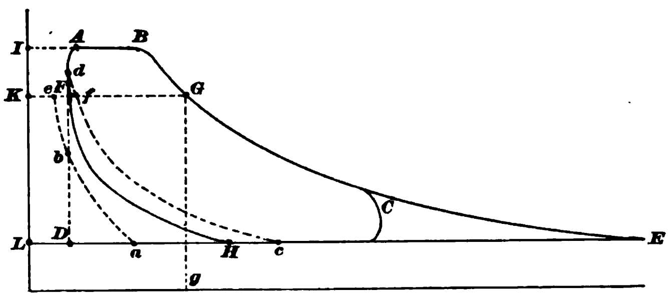


*Fig. 9*

*Solution.* Produce the expansion line $B\ C$, supposed to agree through a sufficient part of its length with a common hyperbola, to the point $E$, where it cuts the back pressure line produced. Find the area added between $C$ and $E$, by any method, and draw a line $F\ G$ cutting off from the top of the card an equal area $A\ B\ G\ F$.

A hyperbolic compression line $F\ H$ drawn through $F$ is the line desired.

*Proof.* Draw two auxiliary hyperbolas $c\ d$ and $a\ e$ near the hyperbola $F\ H$ and consider the areas $K\ G\ E\ L$, etc., in relation to the lines $E\ L$, etc. We shall then have

$$KGEL : FGEH : KFHL : eFHa : FfcH$$
$$= EL : EH : HL : Ha : cH,$$

and if we suppose these areas and lines to be measured by suitable scales the areas will represent quantities of work and the lines the weights of steam performing that work.

$$\text{Then } \frac{ABCHF}{EH} = \frac{FGEH}{EH} = \frac{KGEL}{EL} = \frac{KFHL}{HL} = \frac{eFHa}{Ha}$$
$$= \frac{FfcH}{cH} = V,$$

work done per pound of steam for the card $A\ B\ C\ H\ F$ or $F\ G\ E\ H$.

It remains now to show that the cards $A\ B\ C\ c\ d$ and $A\ B\ C\ a\ b$ are less economical, and this appears easily, as follows:—

Let $\dfrac{ABCcd}{Ec} = v$ = work per pound of the first card; we are to show that $v < V$.

From the above equations we have,

$$\frac{A\,B\,C\,H\,F - F\,f\,c\,H}{E\,H - c\,H} = V, \text{ whereas}$$

$$v = \frac{A\,B\,C\,H\,F - F\,f\,c\,H - F\,f\,d}{E\,H - c\,H}, \text{ therefore}$$

$$v < V.$$

Again, let $\frac{A\,B\,C\,a\,b}{E\,a} = u$; to show that

$$u < V.$$

The equations give

$$\frac{A\,B\,C\,H\,F + e\,F\,H\,a}{E\,H + H\,a} = V, \text{ whereas}$$

$$u = \frac{A\,B\,C\,H\,F + e\,F\,H\,a - F\,e\,b}{E\,H + H\,a}, \text{ therefore}$$

$$u < V.$$

Consequently the most economical compression line is $F\,H$.

When steam is expanded to back pressure, as in the card $F\,G\,E\,H$, the compression should just fill the clearance, and authors generally agree with this.

In such a card, however, any greater compression, as along $c\,f$ (giving the card $f\,G\,E\,c$), is equally economical, though it reduces the capacity of the engine.

Cotterill's expression,

"Greatest work $= P_1\ V_1 \log_e R$",

follows directly from the above, because

$$V = \text{greatest work}$$

$$= \frac{K\,G\,E\,L}{E\,L}; \text{ thus,}$$

$P_1\ V_1 = G\,g \times K\,G$ and $R = \frac{E\,L}{G\,K}$, consequently $K\ G\ E\ L = P_1\ V_1 \log_e R$, in which, if we take $V_1$ as the volume of one pound of steam, $E\,L$ becomes unity and the two expressions identical.

In this method of treating the question, the corners of the card at $A$, $B$ and $C$ may be rounded, as in an actual card, without affecting the accuracy of the results, provided only that the points $F$, $G$ and $H$ are not interfered with, $G\,E$ being a hyperbola and $H\,E$ a horizontal line. Between $G$ and $H$ and between $F$ and $G$, then, the card may have any shape and we have only to make the actual areas $A\ B\ G\ F\ A = G\ E\ H\ C\ G$.

By throttling the steam and increasing the stroke, we may reduce the pressure and change the card $A\ B\ C\ H\ F$ into $F\,G\ E\ H$, with no theoretical loss of economy.

The problem which leads to the equality of the real ratios of expansion and compression is a different, and perhaps more important, one and re-

quires the adjustment of both the expansion and compression so that a given amount of steam may do the most work. This has been solved in a simple manner and will soon be published, but cannot be included in this paper.

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ON THE CHANGES IN THE DIMENSIONS OF METALS WHICH MAY BE DUE TO CHANGES IN MOLECULAR STRUCTURE DEPENDING ON THEIR AGE. By Prof. WM. A. ROGERS, Waterville, Me.

[ABSTRACT.]

THIS paper describes the experiments which have been made every year since 1880 in the determination of the relative lengths of steel, bronze and glass bars.

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IMPROVED FORM OF TRANSMISSION DYNAMOMETER. By Prof. S. W. ROBINSON, Dept. Mech. Eng., Ohio State University, Columbus, Ohio.

[ABSTRACT.]

THIS dynamometer has two pulleys for belts, one to receive and one to transmit the power, and between a weighing device is in application, having a scale beam and weights.

The pulleys are in the same plane, one connected to the other by three gears, one on each pulley axis and one between on a lever whose fulcrum axis is the same as that for the weighing or scale beam. The pulleys are mounted on an arm that will swing around to conveniently receive belts from any direction.

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TEST OF PLANT OF THE HYGEIA ICE COMPANY OF NEW YORK CITY. By A. G. HUPFEL, H. E. GRISWOLD and WM. P. MACKENZIE, New York City, N. Y. [Communicated by Prof. D. S. JACOBUS.]

[ABSTRACT.]

THIS work was undertaken as a graduating thesis for the Stevens Institute of Technology. A continuous test of one week was made and all the measurements were checked by duplicate tests. The plant is one of the best and most improved of its kind and is run in a thoroughly systematic way. The results obtained are therefore especially useful, as they indicate the economy obtained in the best correct practice.

The plant is situated at the foot of East Fifty-second street, New York City. The ammonia compressors and attachments were constructed by the De La Vergne Refrigerating Machine Company.

There are three compressors, each operating two double-acting ammonia compressors. Two of the machines are 60 tons ice-making capacity each and a third 90 tons, giving a total ice-making capacity of 210 tons per

day. The plant was run during the test for twelve hours per day, because the demand at the season of the year at which the test was made was such that it could be met without pushing the plant to its utmost capacity.

The principal results obtained are:

| | | |
|---|---|---|
| Pounds of water evaporated per pound of coal, | | 8.085 |
| Percentage of water fed to boilers used for various purposes. | Engines driving compressors, | 60.1 |
| | Pumps, agitator and elevator engine, | 7.6 |
| | Re-boiler, | 6.5 |
| | Jet blowers to produce forced draught, | 5.6 |
| | Live steam admitted directly to condensers, | 19.7 |
| | Hot water sprinklers to remove ice from cans, | .5 |
| Net ice made per lbs. of coal in lbs., | | 7.12 |
| Percentage of ice lost in removing from cans, | | 2.2 |

[Printed in full in The Stevens Indicator, January, 1894.]

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TESTS OF AUTOMATIC FIRE SPRINKLERS. By Prof. D. S. JACOBUS, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THE various tests adopted by the Department of Tests of the Stevens Institute of Technology to determine the reliability of action and sensitiveness of automatic fire sprinklers were discussed and an improved oven for determining the sensitiveness of the sprinklers described. The oven is one now in use. It was adopted after many experiments with various forms of ovens. The proper form of distribution and other requirements in a sprinkler head were also described in detail.

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AN ACCURATE METHOD OF MEASURING HEAVY LIQUID PRESSURES. By Prof. D. S. JACOBUS, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THIS method was adopted at the suggestion of Professor Webb in measuring the pressure required to burst specimens of steel tubing. A modification of the most accurate method was employed in tests made at a later date. The pressures measured were as high as 25,000 lbs. per square inch. The most accurate method consists of employing a plunger of small diameter which is made to compress a spring by the action of the liquid pressure. An automatic diagram is taken which shows the increase of volume of the tube as the pressure is increased and the corresponding pressures. This diagram resembles the curve of elongations corresponding to given loads obtained in tests of metal by tensile stress and shows that the pressure increases at a nearly uniform rate for a given increase

of volume until a point is reached corresponding to the elastic limit at which the law suddenly changes and the increase of volume for a given increase of pressure becomes much greater.

The second method that has been successfully employed is to use a steel plunger of about ¾″ diameter both to produce and to measure the pressure. The plunger is made to fit accurately so as to be just loose enough to sink into the hole by its own weight. The apparatus is filled with water or oil to within one inch of the top of the hole, and the upper part of the hole is then filled with melted paraffine. After the paraffine cools the steel plunger is inserted. The paraffine prevents leakage, which would be excessive if the plunger acted directly against the water or oil for pressures exceeding 15,000 lbs. per square inch, and does not produce appreciable friction.

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EXPERIMENTAL DETERMINATION OF THE QUICKNESS OF ACTION OF A SHAFT GOVERNOR. By Prof. D. S. JACOBUS, Stevens Institute, Hoboken, N. J.

[ABSTRACT.]

THE experiments were made on the governor of a 50-horse power Ball and Wood engine, and the results of the same are to be used in a paper to be prepared by Mr. F. H. Ball and the writer for the American Society of Mechanical Engineers.

Diagrams 1, 2 and 3 show the path travelled over by the center of the

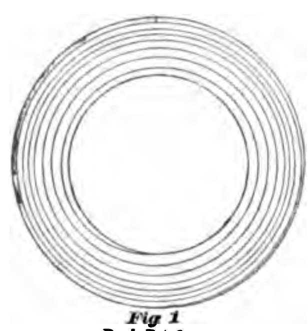

*Fig 1*
*Dash Pot On.*
*Governor Springs set at full Theoretical Tension.*
*Revs. = 285 per Min.*

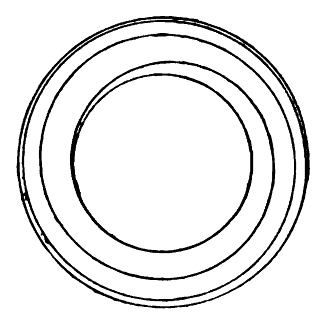

*Fig. 2*
*No Dash Pot.*
*Tension in Springs Adjusted so that Governor is Slightly Unstable.*
*Revs. = 265 per Min.*

valve eccentric when the load is suddenly charged. As the eccentric is moved directly by the governor the number of revolutions required to effect or change in the position of the governor is shown by the number of revolutions to effect a change in the position of the eccentric. Fig. 1 shows the effect of a dast pot in retarding the quickness of action. Fig. 2 shows the path of the weights if the governor is adjusted so as to be

just at the point of unstable equilibrium. Fig. 3 shows the path with the initial tension of the springs made much less than for Fig. 2, so as to give great stability with considerable variation in the number of revolutions of the engine per minute for light and heavy loads.

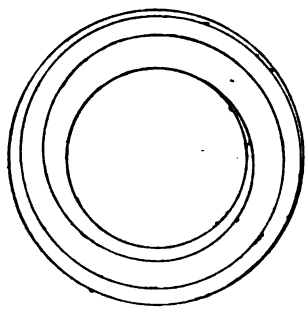

*Fig. 3*
*Tension in Springs Adjusted so that the Governor is in Stable Equilibrium. Revs. - 245 per Min.*

The diagrams show that the dast pot diminishes the quickness of action, so that twice the time is required to move from one position to the other with the dast pot on as is required with no dast pot. It is also shown that the quickness of action is very nearly independent of the stability of

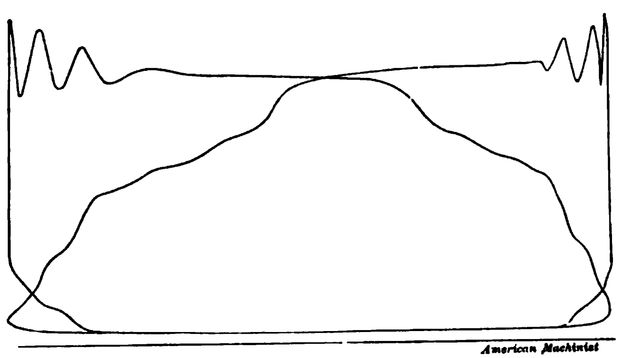


*Fig. 4.*

*Indicator diagram before removing load from the engine. Scale 50 lbs. per inch.*

the governor for, in Fig. 3, where the governor runs at forty revolutions per minute less than the speed for perfect adjustment, the number of revolutions required for the governor to move from one position to the other is the same as in Fig. 2, where the springs are set so that the governor is slightly unstable.

Figs. 4 and 5 show the indicator cards taken before and after changing the load on the engine.

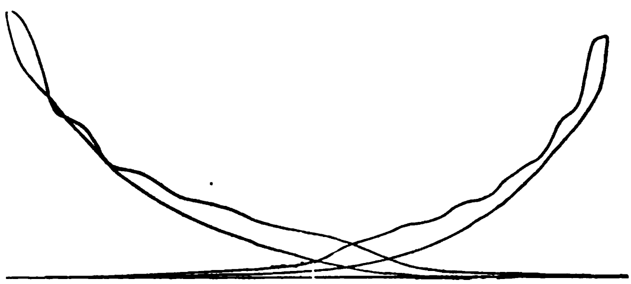

*Fig. 5.*

*Indicator diagram after removing load from the engine. Scale 60 lbs. per inch.*

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A MECHANICAL INDEX. By C. WELLMAN PARKS, U. S. Bureau of Education, Washington, D. C.

[ABSTRACT.]

A NEW plan for indexing current literature intended to overcome some features that have been found objectionable in indexes that are now in use. The plan is intended to avoid the necessity for consulting more than two alphabetical lists to find any article that has been published during the current year.

The index entries will be set in type on a machine similar to the "Linotype." The machine differs in one respect, that is, it sets bold face, italics, and Roman in the same line. Each line of type will be cast solid and can be handled as a card in an ordinary card catalogue.

After a week's index has been printed the type will be placed on suitable galleys and the new matter run in until printing day when the matter will be re-paged and run through the press. The new issue of index will make the previous week's number unnecessary for further use.

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# GEOLOGY AND GEOGRAPHY.

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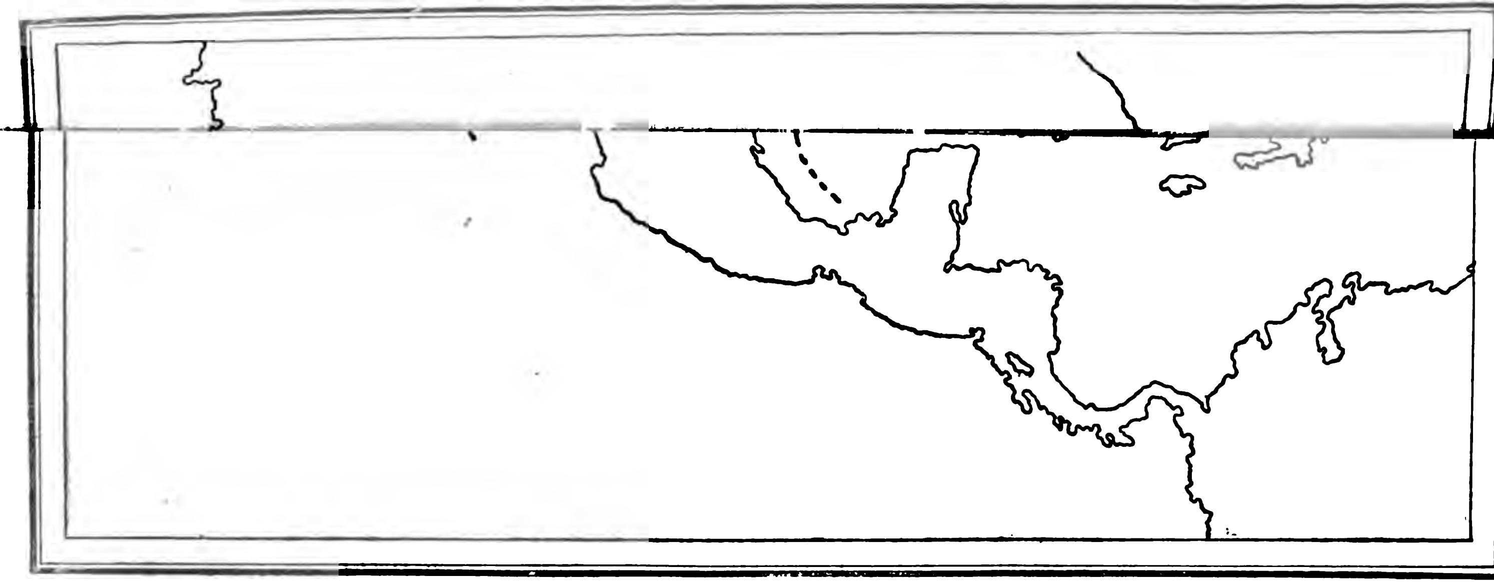

HYPOTHETICAL MAP, TO ILLUSTRATE THE AREAS OF THE CORDILLERAN, MISSISSIPPIAN AND APPALACHIAN SEAS.

# ADDRESS

BY

# CHARLES D. WALCOTT,

VICE PRESIDENT, SECTION E.

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## *GEOLOGIC TIME; AS INDICATED BY THE SEDIMENTARY ROCKS OF NORTH AMERICA.*

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CONTENTS.



DESCRIPTION OF MAP.

On the map the hypothetical areas of the Cordilleran, Mississippi and Appalachian seas are clearly indicated. The land area west of the Cordilleran sea is numbered No. 1. The Californian sea and the area of Paleozoic deposits of western British Columbia, No. 10. The northern extension of the Cordilleran sea (No. 9) is continued as the Paleozoic-devonian sea to the Arctic ocean. The early Cambrian land area (No. 2) east of the Cordilleran sea must have been more or less covered by water during later Paleozoic time. The area now covered by Mesozoic deposits, indicated by No. 3, was presumably covered by the westward and

northward extension of the Paleozoic-Mississippian sea. The area east of the Appalachian sea is indicated by No. 4; and the supposed land barrier between the Hudson Bay and the Mississippian sea by No. 6; it is not improbable that during Ordovician or Silurian time a sea may have connected the two latter seas. The region to the south, indicated by No. 5, is supposed to have been covered by the southward extension of the Appalachian, Mississippian and Cordilleran seas. It is now covered by deposits of Mesozoic and Cenozoic age.

A more detailed description of the map may be gained from the section on the growth of the continent and on the geographic conditions accompanying the different depositions of Paleozoic sediments in the Cordilleran sea.

## INTRODUCTION.

Of all subjects of speculative geology few are more attractive or more uncertain in positive results than geologic time. The physicists have drawn the lines closer and closer until the geologist is told that he must bring his estimates of the age of the earth within a limit of from ten to thirty millions of years. The geologist masses his observations, and replies that more time is required, and suggests to the physicist that there may be an error somewhere in his data or the method of his treatment. The geologist realizes that geologic time cannot be reduced to actual time in decades or centuries; there are too many partially recognized or altogether unknown factors; but he can approximate the relative position of certain formations and by comparison of their sediments, dimensions, and contained record of life with the estimated rates of denudation, sedimentation and organic growth, form a general estimate of their relative time duration. It is my purpose to-day to take up the consideration of the evidence afforded by the sedimentary rocks of our continental area, and largely of a distinct basin of sedimentation, with a view of arriving, if possible, at an approximate time period for their deposition. Before proceeding to examine the conditions of denudation, sedimentation, etc., that enter as factors into the calculation of the age of the earth on the basis of sedimentary geology, we will refer to some of the opinions that have been held by geologists on geologic time and the age of the earth.

Soon after geology emerged from its pre-systematic stage and assumed an independent position among the inductive sciences, speculations on the age of the earth were made by both geologists and physicists. Hutton, Werner, Smith and Cuvier, among the

former, arranged and published their observations and those of their predecessors, during the closing years of the eighteenth century, and in the three succeeding decades rapid progress was made in many lines of investigation by numerous observers, and the literature of geology was enlarged by contributions dealing with nearly every phase of the subject.

*Hutton.* Dr. James Hutton was the founder of physical geology and the predecessor of Lyell in advocating the uniformitarian theory of geology. It is, in a great measure, true—as Lyell has well said of Hutton's attempt to give fixed principles to geology—that "too little progress had been made toward furnishing the necessary data to enable any philosopher, however great his genius, to realize so noble a project."[1] In his first memoir Hutton[2] speaks of a method of measuring the duration of geologic time as follows:

"We are investigating the age of the present earth, from the beginning of that body which was in the bottom of the sea, to the perfection of its nature, which we consider as in the moment of our existence; and we have necessarily another era, which is collateral, or correspondent, in the progress of those natural events. This is the time required, in the natural operations of this globe, for the destruction of a former earth; an earth equally perfect with the present, and an earth equally productive of growing plants and living animals. Now, it must appear, that, if we had a measure for one of those corresponding operations, we would have an equal knowledge of the other."

* * * * * * * * * *

"The highest mountain may be levelled with the plain from whence it springs, without the loss of real territory in the land; but when the ocean makes encroachment on the basis of our earth, the mountain, unsupported, tumbles with its weight; and with that accession of hard bodies, movable with the agitation of the waves, gives to the sea the power of undermining farther and farther into the solid basis of our land. This is the operation which is to be measured; this is the mean proportional by which we are to estimate the age of worlds that have terminated, and the duration of those that are but beginning."

[1] Principles of Geology, 12th Ed., Vol. 1, 1875, p. 73.

[2] Theory of the Earth; or an Investigation of the Laws observable in the Composition, Dissolution and Restoration of Land upon the Globe. Trans. Roy. Soc. Edinburgh, Vol. 1, 1788, pp. 297–298.

He then discusses the data for estimating the length of time it has taken for a specific amount of erosion, and concludes, "that all the coasts of the present continents are wasted by the sea, and constantly wearing away upon the whole; but this operation is so extremely slow, that we cannot find a measure of the quantity in order to form an estimate. Therefore, the present constituents of earth, which we consider as in a state of perfection, would, in the natural operations of the globe, require a time indefinite for their destruction."

He believed that the continents, thus destroyed, were formed from the ruins of preëxisting continents, and that there were records of three such periods, each of which, in our measurement of time, were of indefinite duration.[1]

*Lyell.* In 1830 Sir Charles Lyell began to publish the results of his profound and philosophical studies of geologic phenomena. He firmly established the broad outlines of the law of uniformity as opposed to the doctrine of geologic catastrophies and rendered possible a rough computation of the age of the earth on the principle that geologic processes were the same during geologic time as at the present. Before this effort the scientist and theologian (with the exception of Hutton and his followers) vied with each other in their attempts to harmonize the Mosaic record with that of Nature; they expanded the seventeenth century views of the former and contracted the inductive reasoning from geologic phenomena, and called in the aid of special creations, great catastrophies and other unusual phenomena. This was cleared away among geologists by Lyell's work, supplemented later by the general law of evolution which, since the appearance of Darwin's "Origin of Species" has, with a few and rare exceptions, controlled and directed scientific work and thought in this direction.

Lyell based an argument for the age of the sedimentary rocks as known to him, on the rate of modification of the species of mollusca since the beginning of the "Cambrian period." He divided the geological series into twelve periods and estimated that twenty millions of years were demanded for a complete change in the species of each period, or two hundred and forty million years in all. This estimate excluded the Primordial of Barrande and the "antecedent Laurentian formations."[2]

[1] *Loc. cit.*, p. 304.
[2] Principles of Geology, 10th Ed., Vol. 1, 1867, p. 301.

*Darwin.* In the chapter summing up the imperfections of the geological record Darwin concludes that, if his theory of the origin of species is true, "it is indisputable that before the lowest Cambrian stratum was deposited, long periods elapsed, as long as, or probably far longer than, the whole interval from the Cambrian age to the present day; and that during these vast periods the world swarmed with living creatures." When mentioning the opinions of various authors on the duration of geologic time he indirectly gives his own views, as follows:

"Mr. Croll estimates that about 60,000,000 years have elapsed since the Cambrian period, but this, judging from the small amount of organic change since the commencement of the Glacial epoch, appears a very short time for the many and great mutations of life, which have certainly occurred since the Cambrian formation; and the previous 140,000,000 years can hardly be considered as sufficient for the development of the varied forms of life which already existed during the Cambrian period. It is, however, probable, as Sir William Thompson insists, that the world at a very early period was subjected to more rapid and violent changes in its physical conditions than those now occurring; and such changes would have tended to induce changes at a corresponding rate in the organisms which then existed."[1]

*Houghton.* Rev. Samuel Houghton in commenting on the geological calculus states that he believes that the time during which organic life has existed on the earth is practically infinite. On the basis of the time of cooling he assigns an age of 1,280 millions of years for the Azoic period and remarks that the globe was habital in part at least for a longer period.[2] At a later date when attempting to assign a minor limit to the duration of geologic time, he was driven to the conclusion that geological climates are due to the combined cooling of the earth and sun. On comparing the rates of cooling of such a body as the earth with the maximum measured thickness of the several strata, there is a remarkable proportion between them, which leads towards the conclusion that the maximum thickness of the strata is proportional to the times of their formation. From the combined conclusions deducted from the rate of cooling of the earth and the time required for deposi-

[1] Origin of Species, American Ed., from 6th Eng. Ed., 1882, p. 286.
[2] Manual of Geology, 3rd Ed., 1871, p. 101.

tion of the sedimentary rocks, he gives for the whole duration of geologic time a minimum of two hundred millions of years.[1]

*Croll.* Dr. James Croll began his studies of denudation as a factor in estimates of geologic time in 1865, and reference is made to it in 1867.[2] In the following year a more elaborate paper was published and subsequently numerous references have been made to it and other factors that are to be considered in the estimate of geologic time.[3] Dr. Croll agrees with Sir W. Thompson that Professor Tait probably overestimated the time when he affirms that 10,000,000 years is about the utmost that can be allowed, from the physical point of view, for all the changes that have taken place in the earth's surface since vegetable life of the lowest known form was capable of existing there. He remarks, "And this is certainly all that ever can be expected from gravitation; mathematical computation has demonstrated that it can give no more. The other theory, founded on motion in space, a cause as real as gravitation, labors under no such limitation. According to it, so far at least as regards the store of energy which may have been possessed by the sun, plant and animal life may date back, not to 10,000,000 years, but to a period indefinitely more remote. In fact, there is as yet no limit to the amount of heat which this cause may have produced; for this depended upon the velocities of the two bodies at the moment prior to collision, and what these velocities were we have no means of knowing. They might have been five hundred miles a second, for anything which can be shown to the contrary. Of course, I by no means affirm that it is as much as 100,000,000 years since life began upon our earth, but I certainly do affirm that, in so far as a possible source of the sun's energy is concerned, life may have begun at a period as remote.[4]

Dr. Croll considers the geological evidence relating to the age of the sun's heat on the principle that "in order to determine the present rate of subareal denudation, we have only to ascertain the quantity of sediment annually carried down by the river systems.[5] After extended consideration of the evidence he concludes that a period of 24,000,000 years would be required for the deposition of

[1] Nature, Vol. 18, 1878, pp. 267–268

[2] Phil. Mag. London, Vol. 33, 1867, p. 130.

[3] Phil. Mag. London, Vol. 35, 1868, pp. 363–384; Vol. 36, 1868, pp. 362–386; Geol. Mag. London, 1871, pp. 97–102. Climate and Time, London, 1875, pp. 329–367. Stella Evolution and its Relations to Geological Time, 1889, pp. 39–68.

[4] Evolution and its Relation to Geological Time, 1889, p. 36.

[5] *Loc. cit.*, p. 39.

the known sedimentary rocks. On the theory that the present existing sedimentary rocks have on the average passed at least twice through the cycle of destruction and reformation the period is multiplied by three, which results in 72,000,000 years for the time of duration since the beginning of the deposition of the sedimentary rocks. He says further, "It is impossible to tell from geological data the actual age of the stratified rocks; but this is not required. What we require is, as already stated, not their actual age but an inferior limit to that age."

*Wallace.* The chapter on "The earth's age," contained in Sir A. R. Wallace's "Island life," is an admirable summary of his own views and those of various geologists, naturalists, and physicists who have written on the subject. From the consideration of data bearing on the denudation and deposition of strata as a measure of time he thinks that 28,000,000 years will be sufficient for the deposition of the known sedimentary rocks. Of the value of this estimate, he says, "It is not, of course, supposed that the calculation here given marks any approach to accuracy, but it is believed that it does indicate the order of magnitude of the time required. We have a certain number of data, which are not guessed but the result of actual measurement; such are, the amount of solid matter carried down by rivers, the width of the belt within which this matter is mainly deposited, and the maximum thickness of the known stratified rocks.[1] By adopting Croll's theory of glacial epochs occurring at certain periods of great eccentricity, several datum points are secured by Wallace that are correlated with certain geologic phenomena of the Tertiary and Pleistocene periods and the probable date of the Miocene period. He then takes the ratio of Lyell for the duration of the geologic epochs and concludes that sixteen millions of years have passed since Cambrian time. On the basis of Dana's theory that the Tertiary is only one-fifteenth of the Mesozoic and Paleozoic combined, the result is sixty millions of years for the same interval. Of these figures, he says: "The estimate arrived at for the rate of denudation and deposition (twenty-eighty million years) is nearly midway between these and it is, at all events, satisfactory that the various measures result in figures of the same order of magnitude, which is all one can expect on so difficult and exceedingly speculative a subject. The only value of such estimates is to define our notions of geological

[1] Island Life, 2d Ed., 1892, pp. 222, 223.

time, and to show that the enormous periods of hundreds of millions of years, which have sometimes been indicated by geologists, are neither necessary nor warranted by the facts at our command; while the present result places us more in harmony with the calculation of physicists, by leaving a very wide margin between geological time as defined by the fossiliferous rocks, and that far more extensive period which includes all possibility of life upon the earth."[1]

The results obtained by Wallace are questioned by T. Mellard Reade, who states that Wallace has not allowed for the erosion and redeposition of the same sediments a number of times.[2]

*Winchell.* Dr. Alexander Winchell reviews the opinions of physicists and geologists on the age of the world or of certain periods, and enumerates the grounds for the various estimates, as follows:

(1) The time required for the sun to contract from a nebulous condition, or from the orbit of the earth to its present limits.

(2) The time which the sun will require to cool from its present condition to a darkened or planetary state.

(3) The time required for the earth to cool from incipient incrustation to its present state, based on the thermal conductivity of rock masses and the rate of increased heat towards the earth's centre.

(4) Relative times required for the deposition of all the rocky sediments.

(5) Calculation based on the obliteration of the rotational effects of the upheaval of a continental mass.

(6) The time since the middle of the last glacial period, based on the theory that epochs of glaciation on the northern hemisphere have been caused by extreme eccentricity of the earth's orbit.

(7) Estimates based on rates of erosions and depositions.

(8) The rate of bluff-recession and terrace-formation.

(9) Decrease of temperature of ground covered by ice during the glacial period, as compared with temperature of ground not chilled by the ice sheet.[3]

Doctor Winchell was inclined to accord at least equal confidence to the later results of geologic action, such as erosion of river gorges and lakeside and seaside bluffs, as he would give to the

[1] *Loc. cit.*, pp. 235, 236.

[2] Geol. Mag., Vol. 10, 1883, pp. 309–310.

[3] World Life, or Comparative Geology. Chicago, 1883, pp. 355–376.

mathematical methods of the physicist. On this basis he deduced the result that the whole incrusted age of the world would be 3,000,000 years. In conclusion, he says:

"If our attempts to ascertain the age of the world, or the duration of any single period of its evolution, yield only uncertain results, they suffice at least to demonstrate that geological history has limits far within the wild conceptions of a certain class of geologists. They show, if we may credit the indications here regarded most trustworthy, a restriction of the modern epoch within limits not exceeding one-tenth or one-twentieth the duration sometimes assigned to it."[1]

*Geikie.* Sir Archibald Geikie has recently summed up the case of the geologist and physicist in a very clear statement, as follows:

"In scientific as in other mundane questions there may often be two sides, and the truth may ultimately be found not to lie wholly with either. I frankly confess that the demands of the early geologists for an unlimited series of ages were extravagant, and even, for their own purposes, unnecessary, and the physicist did good service in reducing them. It may also be freely admitted that the latest conclusions from physical considerations of the extent of geological time require that the interpretation given to the record of the rocks should be vigorously revised, with the view of ascertaining how far that interpretation may be capable of modification or amendment. But we must also remember that the geological record constitutes a voluminous body of evidence regarding the earth's history which cannot be ignored, and must be explained in accordance with ascertained natural laws. If the conclusions derived from the most careful study of this record cannot be reconciled with those drawn from physical considerations, it is surely not too much to ask that the latter should be also revised. It has been well said that the mathematical mill is an admirable piece of machinery, but that the value of what it yields depends upon the quality of what is put into it. That there must be some flaw in the physical argument, I can, for my own part, hardly doubt, though I do not pretend to be able to say where it is to be found. Some assumption, it seems to me, has been made, or some consideration has been left out of sight, which will eventually be seen to vitiate the conclusions, and which, when duly taken into account,

[1] *Loc. cit.*, p. 378.

will allow time enough for any reasonable interpretation of the geological record."[1]

Of the rate of denudation and deposition he says:

"The rate of deposition of new sedimentary formations, over an area of sea-floor equivalent to that which has yielded the sediment, may vary from one foot in 730 years to one foot in 6,800 years. If now we take these results and apply them as measures of the length of time required for the deposition of the various sedimentary masses that form the outer part of the earth's crust, we obtain some indication of the duration of geological history. On a reasonable computation these stratified masses, where most fully developed, attain a united thickness of not less than 100,000 feet. If they were all laid down at the most rapid recorded rate of denudation, they would require a period of seventy-three millions of years for their completion. If they were laid down at the slowest rate they would demand a period of not less than 680 millions."[2]

*Reade.* Mr. T. Mellard Reade has been a large contributor to the literature of geologic time, both directly and indirectly. His most recent conclusion is that there appears to be a concensus of opinion that one foot in 3,000 years is a fair estimate of the mean rate of such areal erosion over all land areas throughout all geological time. The calculation that has elapsed since the beginning of Cambrian time, on this basis, is stated as follows:

"The mean area of denudation throughout post-Archean times being taken as one-third the entire land areas of the globe, the bulk of the post-Archean rocks being expressed by the land area of the globe two miles thick, and the rate of denudation one foot in 3,000 years, the time of accumulation will be $5280 \times 2 \times 3000 \times 3 = 95,040,000$. The time that has elapsed since the commencement of the Cambrian is therefore in round figures 95,000,000 of years."[3]

Speaking of Sir Archibald Geike's conclusion that the earth's age, geologically speaking, must be somewhere between 100,000,000 and 600,000,000 of years, he says:

"This is a large margin no doubt, but it is an important thing to know. Different men may put different value on the three factors, bulk of sediment, rate of denudation, and area of denudation;

[1] Presidential Address. Report Sixty-second Meeting Brit. Assoc. Adv. Sci., 1892, pp. 19-20.

[2] *Loc. cit.*, p. 21.

[3] Measurement of Geological Time. Geol. Mag., Vol. 10, 1893, pp. 99-100.

but I think a fair and impartial examination of the reasoning involved in this paper will show that the principle of the calculation is sound."

"It must not be forgotten that to arrive at the earth's age Archean time has to be added to my estimate of 95,000,000 years, which very materially increases the margin of geologic time on which we are allowed to draw."[1]

In an earlier paper Reade assembles much valuable data on chemical denudation,[2] and later reviews the results obtained by the geologist and the mathematician.[3]

M. A. de Lapparent is one of the few European continental geologists who have written on geologic time. On the basis of mechanical denudation and sedimentation he thinks that from 67,000,000 to 90,000,000 of years, at the present rate of sedimentation, would account for everything that has been produced since the consolidation of the crust.[4]

*Dana.* In some observations on the length of geologic time Prof. James D. Dana says that geology has no means of substituting positive lengths of time in place of the time ratios he had deduced from the relative thicknesses of the rock series pertaining to the several geologic ages, but that it affords facts sufficient to prove the general proposition, that geologic "*time is long.*" He cites examples, such as the retreat of Niagara Falls and the recent growth of coral reefs. According to his time-ratio, if 48,000,000 years is assigned since the commencement of the Silurian, the Paleozoic, Mesozoic, and Cenozoic time would give for each severally, 36,000,000, 9,000,000 and 3,000,000 years."[5]

*McGee.* In an article on "Comparative chronology" by Mr. W J McGee the conclusion is reached that the antiquity of the glacial deposits margined by the great terminal moraine is about 7,000 years, and of the Columbian formation and of the Ice invasion to which it is ascribed, 200,000 years; and of the Lafayette formation of late Tertiary age, 10,000,000 years. On this basis the mean estimate of the age of the earth is 15,000,000,000 years

[1] *Loc. cit.*, p. 100.

[2] Proc. Liverpool Geol. Soc., Vol. 3, pl. iii, 1877, pp. 211–235.

[3] Geol. Mag., Vol 5, 1878, pp. 145–154.

[4] De la mesure du temps, parles phénomènes de sédimentation. Bull. Géol. Soc. France, 3d ser., Vol. 18, 1890, pp. 351–355

La Histirée de la terre ferme et durée des temps géologiques. Revue des questions scientifiques. July, 1891, Pamphlet, Bruxelles, pp. 1–38.

[5] Manual of Geology, 2nd Ed., pp. 590–591.

and 7,000,000,000 since the beginning of Paleozoic time.[1] In a subsequent "Note on the Age of the Earth" Mr. McGee modifies his former statement and gives as a mean estimate of the age of the earth, 6,000,000,000 years and of the duration of time since the beginning of the Paleozoic, 2,400,000,000 years,—which is based on a minimum estimate for the age of the earth of 10,000,000 years and a maximum estimate of 5,000,000,000,000 years.[2] Mr. McGee, in speaking of these estimates, says: "These general estimates are indefinite, and the minima, mean, and maxima are alike unworthy of final acceptance; but they stand for a real problem and not a merely ideal one, and represent actual conditions of the known earth; and, so far as the science of geology is concerned, the maximum estimate is quite as probable as the minimum, while the mean is much more probable than either."[3]

*Upham.* Prof. Warren Upham, after reviewing various estimates of geologic time, concludes that the "probable length of Glacial and Post-Glacial time together is 30,000 or 40,000 years, more or less; but an equal or considerably longer preceding time, while the areas that became covered by ice were being uplifted to high altitudes, may perhaps with good reason be also included in the Quaternary era, which then would comprise some 100,000 years." He then applies Professor Dana's time-ratios and concludes that the time needed for the earth's stratified rocks and the unfolding of its plant and animal life must be about a hundred millions of years.[4] Mr. Upham's paper gives a number of illustrations of geologic phenomena from Tertiary and Pleistocene geology that bear upon the time duration of these epochs.

From the foregoing estimates of geologic time the only conclusion that can be drawn is that the earth is *very old* and that man's occupation of it is but a day's span as compared with the eons that have elapsed since the first consolidation of the rocks with which the geologist is acquainted.

When I began the preparation of this paper it was my intention to carefully analyze the sedimentary rocks of the entire geologic series as exposed upon the North American continent. I soon found, however, that the time at my disposal would make this im-

[1] Am. Anthropologist, Vol. 5, 1892, p. 340.
[2] Science, Vol. 21, 1893, p. 309.
[3] *Loc. cit.*, p. 310.
[4] Am. Journal Sci., Vol. 45, 1893, pp. 217–218.

practicable, and I decided to take up the history of the deposits that accumulated in Paleozoic time on the western side of our continent, in an area that for convenience I shall call the Cordilleran sea.[1] This was chosen (1) as I was personally acquainted with many of its typical sections; (2) there was a broad and almost uninterrupted sedimentation during Paleozoic time, and (3) there was a prospect for obtaining more satisfactory data as a basis of calculation, since calcareous deposits are in excess of those of mechanical origin.

We will now consider certain points in relation to the growth or evolution of the North American continent, as the deposition of mechanical sediments depends, to a considerable extent, on the character of the adjoining land area and chemical sedimentation is also influenced by it.

### GROWTH OF THE CONTINENT.

The Algónkian sediments were deposited in interior and bordering seas that filled the depressions and extended over the margins of the Archean continent. From the great thickness of mechanical sediments it was evidently a period of elevated land and rapid denudation. With the close of Algonkian time extensive orographic movements occurred that outlined the subsequent development of the continent. The lines of the Rocky mountain and Appalachian ranges were emphasized and the great basins of sedimentation west of them defined. Subsequent movements have elevated the old and formed new sub-parallel ranges. These movements were often of long duration and also separated by great intervals of time, as is shown by the long continued base levels of erosion during which the great thickness of calcareous deposits accumulated in the Cordilleran and Appalachian seas. Since Algonkian time the growth of the continent has been by the deposition of sediments in the bordering oceans and interior seas and lakes within the limits of the continental plateau; and it is considered that the relative position of the continental plateau and the deep sea has not materially changed during that period. How much the deposits on the continental border have increased its area is unknown, as at present they are largely concealed beneath the waters of the ocean. During Paleozoic time the two areas of greatest known accumulation were in the Appalachian and Cordilleran seas, where 30,000

[1] See p. 155.

feet or more of sediments were deposited. In the Cordilleran sea sedimentation was practically uninterrupted (except during a short interval in middle Ordovician time) until towards the close of Paleozoic time. In the northern Appalachian sea it continued without any marked unconformity, from early Cambrian to the close of Ordovician time and, south of New York, with relatively little interruption, until the close of Paleozoic time. Certain minor disturbances occurred along the eastern border of the sea, but they were not of sufficient extent to effect a general conclusion,—which is that the depression of areas of deposition within the continental platform continued without reversal of the subsidence during Paleozoic time. During Cambrian and, it may be late Algonkian time, the extended interior Mississippian region was practically levelled by denudation, the eroded material being carried into the Cordilleran and Appalachian seas and, probably, to a sea to the south.

The sedimentation of the Mississippian area in Paleozoic time, between the Appalachian and the Cordilleran seas, was small as compared to that which accumulated in the latter. In Devonian time there does not appear to have been any sedimentation in the western portion of it west of the ninety-fourth meridian and east of the Cordilleran sea, and it was slight in the same interval in the Appalachian sea south of the thirty-seventh parallel.[1] There is little, if any evidence, in the sediments of Paleozoic time to show that they were deposited in the deep, open ocean; on the contrary, they were largely accumulated in partially enclosed seas or mediterraneans and on the borders of the continental plateau. The former is particularly true of the sedimentation of the Cordilleran and Appalachian seas and the broad Mississippian sea.

The close of the prolonged period of Paleozoic sedimentation was brought about by what Dana has termed the "Appalachian revolution." The topography of the continent was more or less changed, and the conditions of sedimentation that followed were unlike those that preceded. This revolution raised above the sea level a considerable portion of the Cordilleran and the Appala-

[1] The non-occurrence of Devonian sediment has not yet been fully explained. It has been suggested that the sea beyond the reach of mechanical sedimentation was too deep for the deposition of calcareous deposits. It is more probable that the sea was shallow and an area of non-deposition, or that its bed was raised to form a low, level land surface at a base level of erosion that was subjected to very slight degradation.

chian sea beds and also of the Mississippian sea, east of the ninety-sixth meridian and north of the thirty-fourth parallel. In its effect it may be compared to the Algonkian revolution[1] that preceded the deposition of the Paleozoic sediments.

With the opening of new conditions the sedimentation of Mesozoic time began upon the Atlantic border and over large areas of the western half of the continent with the deposit of mechanical sediments—sands, silts, etc.,—during Jura-Trias time. They are of a character that naturally follows a period of disturbance of preëxisting conditions and the formation of new basins of deposition with more or less elevated adjoining land areas. At its close orographic movements affecting the positions of the beds occurred upon the Pacific and Atlantic coasts and also, to a more limited degree, throughout the Rocky mountain region. This does not appear to have extended over the plateau region or the central belt between the ninety-seventh and one hundred and fifth meridians.

The Cretaceous formations have their greatest development between the ninety-seventh and one hundred and twelfth meridians in Mexico and the United States, in a broad belt which extends from the boundary of the latter to the northwest into the British possessions as far as the sixty-first parallel. They were of a marine origin until towards the close of the period when a prolonged orographic movement elevated a large area of the continent above sea level and locally upturned the Cretaceous strata in the Rocky mountain area. The shoaling of the sea was followed by the formation of great inland lakes in which fresh water deposits succeeded the marine and estuarian sediments. Over the coastal regions they were of marine origin throughout.

The Tertiary sediments deposited on the Cretaceous are marine on the Atlantic, Gulf of Mexico and Pacific coasts, and of fresh-water origin in the Rocky mountains and Great Plains areas—where they were deposited in the great inland lakes outlined in the previous period.

## GEOGRAPHIC CONDITIONS ACCOMPANYING THE DEPOSITION OF PALEOZOIC SEDIMENTS IN THE CORDILLERAN SEA.

The assumed area of the Cordilleran or Paleo-Rocky Mountain

[1] The term "revolution" is used to describe the culmination of a long series of phenomena that finally resulted in a distinctly marked epoch in the evolution of the continent. The "Appalachian revolution" began far back in the Paleozoic, and culminated in the latter stages of the Carboniferous, and the Algonkian revolution probably began far back in Algonkian time.

sea includes over 400,000 square miles between the thirty-fifth and fifty-fifth parellels. To the eastward, during lower and middle Cambrian time, a land area is thought to have extended from east of the one hundred and eleventh meridian across the continent to the Paleo-Appalachian sea. This land was depressed toward the close of middle Cambrian time, and the Mississippian sea expanded over the wide plateau-like interior region, from the Gulf of Mexico on the south to the Lake Superior region on the north; westward it penetrated among the mountain ridges between the one hundred and fifth and one hundred and eleventh meridians, laying down the upper Cambrian deposits that are now found in New Mexico, Arizona, eastern Utah, the western half of Colorado, Wyoming, Idaho and Montana, and still further north into Alberta and British Columbia. During Ordovician, Silurian, Devonian and Carboniferous time this entire Mississippian region, except portions in Devonian time, appears to have been covered by a relatively shallow sea that was coëxtensive with the Appalachian sea and that communicated freely with the Cordilleran sea. During this same age, however, the Rocky mountain area of New Mexico, Colorado, Utah, Wyoming and Montana formed a more or less well defined boundary of ridges and islands between the Cordilleran and the interior sea up to the forty-ninth parallel. To the north of the latter the conditions appear to have been the same as on the eastern side of the continent, where the Appalachian sea communicated freely with the Mississippian sea. From the data that we now have I think that the Paleozoic (Mississippian) sea extended at times over nearly all of the area subsequently covered by the Cretaceous and the later formations between the Gulf of Mexico and the Arctic ocean. This belt is bounded almost continuously on the east and west by Paleozoic rocks that extend from the Arctic ocean to Mexico, and whether of Cambrian, Ordovician, Silurian or Devonian age they carry essentially the same fauna throughout their extent. In the outcrops of lower strata that rise up through this Cretaceous area, the Cambrian, Ordovician and Carboniferous rocks are found encircling the pre-Paleozoic rocks. Instances in which the Archean rocks have been met with immediately beneath the Cretaceous in borings in Dakota and Minnesota are along the eastern border of the area, next to the Archean rocks,—where it is probable that the Cretaceous overlaps the Paleozoic to the Archean.

The western side of the Cordilleran sea seems to have been bounded by a land area that separated it from the Paleozoic sea, which extended through central California and the Pacific border of British Columbia and Vancouver's Island. From the position of the Carboniferous deposits of California at the present time it appears that this land varied from 100 to 150 miles in width and was practically continuous along the western side of the Cordilleran sea. This view is further strengthened by the fact that the Carboniferous fauna of California has certain characteristics which are not found in the Carboniferous of the Cordilleran area. Our knowledge of the conditions north of the fifty-fifth parallel is limited by the want of accurate geologic data. If Cambrian and Carboniferous rocks were not deposited in the Mackenzie river basin and also on the eastern side of the area now covered by Cretaceous strata, the inference is that during Cambrian and Carboniferous time there was a land area to the east and north of the northern Cordilleran sea that may have been tributary to the latter.

### SOURCE OF SEDIMENTS DEPOSITED IN THE CORDILLERAN SEA.

The sediments deposited in every sea or lake are derived from land areas either by mechanical or chemical denudation.

Mechanical denudation results from the action of the waves and currents along the shore and the agency of rain, frost, snow, ice, wind, heat, etc., on the land. Rain is the most important factor and the result depends mainly upon its amount and the slope or the gradient of the land. The general average of denudation for the surface of the land areas of the globe, now usually accepted, is one foot in 3,000 years. This varies locally, according to Sir Archibald Geikie, from one foot in 750 years to one foot in 6,000 years.[1] Of the rate of denudation during Paleozoic time about the Cordilleran sea we know very little, but I think that it was relatively rapid in early Cambrian time and during the deposition of the arenaceous sediments of the Ordovician and Carboniferous.

The material forming the argillaceous shales of the Cambrian and Devonian was supplied to the sea more slowly. These conclusions are sustained by the slight change in the character of the faunas where interrupted by the sands and pebbles of the Ordovician and Carboniferous and the marked change between the base and summit of the argillaceous shales. As a whole I

[1] Brit. Assoc. Adv. Sci., Sixty-Second Meeting, 1893, p. 21.

think we are justified in assuming a minimum rate of mechanical denudation—of considerably less than one foot in one thousand years—for the area tributary to the Cordilleran sea.

Chemical denudation is the removal of material taken into solution by water. Mr. T. Mellard Reade has discussed this phase of denudation in an admirable manner.[1] He came to the conclusion from what was known of the volume of water discharged into the ocean per year, the average amount of material in chemical solution, and the area of land surface drained by the rivers, that an average of 100 tons of rocky matter is dissolved per English square mile per annum. Of this he says: "If we allot 50 tons to carbonate of lime, 20 tons to sulphate of lime, 7 to silica, 4 to carbonate of magnesia, 4 to sulphate of magnesia, 1 to peroxide of iron, 8 to chloride of sodium, and 6 to the alkaline carbonates and sulphates we shall probably be as near the truth as present data will allow us to come."[2] By the use of the data given by Mr. John Murray, in a paper on "The total annual rainfall on the land of the globe, and the relation of rainfall to the discharge of rivers,"[3] I obtain 113 tons as the total amount of matter in solution discharged into the Atlantic basin per annum from each square mile of area drained into it. Of this 49 tons consist of carbonate of lime and 5.5 tons of sulphate and phosphate of lime.[4]

*Mechanical sediments.* With the geographic conditions described as prevailing during Paleozoic time, the source of mechanical sediments later than the Middle Cambrian must have been from the broken area on the eastern side that extended 100 to 200 miles to the eastward and to a much greater extent from the land along the western side of the sea. The enormous deposit of from 10,000 to 20,000 feet of mechanical sediments in early Cambrian time is explained by the assumption of favorable topographic conditions of denudation following the Algonkian revolution and the presence of a land area over the interior portion of the continent and also, in all probabilty, between the western side of the Cordilleran sea

[1] Proc. Liverpool Geol. Soc., Vol. 3, 1877, pp. 212–235. Chemical Denudation in Relation to Geological Time. 1879, pp. 1–61.

[2] *Loc. cit.*, p. 229.

[3] Scottish Geol. Mag., Vol. 3, 1887, pp. 65–77.

[4] Total amount removed in solution per annum by rivers, 762,587 tons per cubic mile of river water. Total discharge of river water per annum into the Atlantic, 3.947 cubic miles. Area drained, 26,400,000 square miles. Amount of carbonate of lime per annum, 326,710 tons per cubic mile of river water; of sulphate and phosphate of lime, 37,274 tons.

and the western border of the continent. During this period the conformable pre-fossiliferous strata of the Cambrian accumulated and about 6,000 feet of the lower fossiliferous rocks as they occur in the Eureka district of central Nevada. Following the depression of the continent, which carried down the central area and also introduced the upper Cambrian (Mississippian) sea into the Rocky mountain area of Colorado, etc., there were deposited of mechanical sediments in central Nevada:

| | |
|---|---|
| Ordovician sands | 500 feet. |
| Devonian fine argillaceous muds | 2,000 " |
| Lower Carboniferous sands | 3,000 " |
| Upper Carboniferous conglomerate and sands | 2,000 " |
| | 7,500 " |

making a total of 7,500 feet of mechanical sediments, the remaining portion of the section (15,150 feet) being limestone.

The following table exhibits the relative thickness of mechanical and chemical deposits in the Cordilleran sea, after the middle Cambrian subsidence:

| | Wahsatch. | Central Nevada. | Southwest Nevada. | Montana. | Alberta. |
|---|---|---|---|---|---|
| Mechanical sediment. | 10,000 | 7,500 | 2,500 | 1,000 | 4,600 |
| Chemical sediment. | 10,400 | 15,150 | 13,000 | 4,000 | 15,000 |
| Ratio | $\frac{1}{1}$ | $\frac{1}{2}$ | $\frac{1}{5}$ | $\frac{1}{4}$ | $\frac{1}{3}$ |

If an average is taken of the mechanical sediment deposited subsequent to the close of middle Cambrian time, it will be found to be about 5,000 feet for the entire area, which, I think, does away with any necessity to assume an additional hypothetical land area for the source of mechanical sediment. The fine sand composing the quartzites and the silt forming the shales, as well as the fine conglomerate of later deposits, were derived from the adjoining land areas, and, in all probability, currents swept through from the ocean to south or north, distributing the mud and sand contributed from the rivers and streams along the shores.

*Chemical sediments.* The present supply of the carbonate of lime, silica, etc., contained in sea-water is derived from waters poured into the sea by rivers and streams. The Cordilleran sea undoubtedly received a large contribution from the adjoining land areas, but a considerable amount was possibly derived from an oceanic current that circulated through it as the southern equa-

torial current of the Atlantic now sweeps through the Caribbean. From the vast deposits of carbonate of lime it might be assumed, *a priori*, that the waters of a Mississippi or Amazon were poured into it, but there is not any evidence of the existence of such a river, although the tributary area may have been very large in Cambrian and Carboniferous time, if the drainage of the country west of Hudson's Bay was to the westward.

*Conditions of deposition.* With free communication into the open ocean on the south, and probably on the north, during most of Paleozoic time strong currents must have circulated through the Cordilleran sea. The broad distribution of mechanical sediments of uniform character clearly shows this to have been the case, especially in pre-Silurian time. The present known distribution of the mechanical sediments indicate that they were mainly brought into the sea from the west,[1] although a vast amount was derived from the land on the eastern side in pre-Ordivician time; they were quite evenly distributed over the sea bed, except where local accumulations of silt and sand occurred near the larger sources of supply, or in the direction of powerful currents within the sea.

The conditions of the deposition of the carbonate of lime are less clearly understood than those governing mechanical sediments, and I shall enter upon the discussion of them at considerable length. There are three methods by which it usually is considered that it may be deposited: 1. Agency of organisms. 2. Chemical precipitation. 3. By mechanical methods.

It is the general opinion of geologists that limestone rocks are the result almost entirely of the consolidation of lime removed from the sea water through the agency of life, and that they consist of the remains of foraminifera, crinoids, corals, etc., or their fragments, embedded in a more or less crystalline matrix resulting from subsequent alteration of the original deposits. This, however, has been seriously questioned. Sorby, in giving his general conclusions of an extensive microscopic examination of limestones states that:

"Even if it were possible to study in a detached state the finer granular particles which constitute so large a part of many limestone formations, it would usually be impossible to say whether they had been derived from organisms which can decay down into granules, or from other organisms which can only be worn down

[1] Geol. Expl. Fortieth Parallel, Vol. 1, 1878, p. 247.

into granules, or from ground-down older limestone, or, in some cases, from carbonate of lime deposited chemically as granules. . . . The shape and character of the identifiable fragments do, indeed, *prove* that much of this must have been derived from the decayed and worn-down calcareous organisms; and very often we may reasonably *infer* that the greater part, if not the whole, was so derived; but at the same time, it is impossible to *prove*, from the structure of the rock, whether some, or how much, was derived from limestones of earlier date, or was deposited chemically, as some certainly must have been."[1]

In their memoir on coral reefs and other carbonate of lime formations in modern seas, Messrs. Murray and Irvine show that temperature of the water has a controlling influence upon the abundance of species and individuals of lime-secreting organisms; high temperature is more favorable to abundant secretion of carbonate of lime than high salinity.[2]

Taking the samples of deep-sea deposits collected by the Challenger as a guide, the average percentage of carbonate of lime in the whole deposit covering the floor of the ocean is 36.83; of this it is estimated that fully 90 per cent is derived from pelagic organisms that have fallen from the surface water, the remainder of the carbonate of lime having been secreted by organisms that laid on, or were attached to, the bottom. The estimated area of the various kinds of deposits, the average depth and the average percentage of carbonate of lime to each are shown in the following table:

TABLE SHOWING THE ESTIMATED AREA, MEAN DEPTH, AND MEAN PERCENTAGE OF $CaCO_3$, OF THE DIFFERENT DEPOSITS.[3]

| Deposit. | | Area, square miles. | Mean depth in fathoms. | Mean per cent. of $CaCO_3$ |
|---|---|---|---|---|
| Oceanic oozes and clays ... | Red clay............ | 50,289,600 | 2,727 | 6.70 |
| | Radiolarian ooze... | 2,790,400 | 2,894 | 4.01 |
| | Diatom ooze........ | 10,420,600 | 1,477 | 22.96 |
| | Globigerina ooze... | 47,752,500 | 1,996 | 64.53 |
| | Pteropod ooze...... | 887,100 | 1,118 | 79.26 |
| Terrigenous deposits.... | Coral sands and muds.............. | 3,219,800 | 710 | 86.41 |
| | Other terrigenous deposits, blue muds, etc......... | 27,899,300 | 1,016 | 19.20 |

[1] Quart. Jour. Geol. Soc. London, Vol. 35, 1879, pp. 91-92.
[2] Proc. Royal Soc., Edinburgh, Vol. 17, 1890, p. 81.
[3] *Loc. cit.*, p. 82.

"We have little knowledge as to the thickness of these deposits, still such as we have goes to show that in these organic calcareous oozes and muds, we have a vast formation greatly exceeding in bulk and extent the coral reefs of tropical seas; they are most widely distributed in equatorial regions, but some patches of Globigerina ooze are to be found even within the Arctic circle, in the course of the Gulf Stream."[1]

The percentage of carbonate of lime contained in deposits accumulating at different depths, as obtained from two hundred and thirty-one samples collected by the Challenger, is shown in the following tabulation:

| | | | | | | |
|---|---|---|---|---|---|---|
| "14 | cases | under | 500 | fathoms, | m. p. c. | 86.04 |
| 7 | " | " | 500 to 1,000 | " | " | 66.86 |
| 24 | " | " | 1,000 to 1,500 | " | " | 70.87 |
| 42 | " | " | 1,500 to 2,000 | " | " | 69.55 |
| 68 | " | " | 2,000 to 2,500 | " | " | 46.73 |
| 65 | " | " | 2,500 to 3,000 | " | " | 17.36 |
| 8 | " | " | 3,000 to 3,500 | " | " | 0.88 |
| 2 | " | " | 3,500 to 4,000 | " | " | 0 00 |
| 1 | " | " | 4,000 | " | " | trace." |

The fourteen samples under 500 fathoms are chiefly coral muds and sands, and the seven samples from 500 to 1,000 fathoms contain a considerable quantity of mineral particles from continents or volcanic islands. In all the depths greater than 1,000 fathoms the carbonate of lime is mostly derived from the shells of pelagic organisms that have fallen from the surface waters, and it will be noticed that these wholly disappear from the greater depths.[2]

By a series of experiments Messrs. Murray and Irvine found: "That although sea water under certain conditions may take up a considerable quantity of carbonate of lime in solution, yet it is unable permanently to retain in solution more than is usually found to be present in sea water, and it is owing to this that the amount of carbonate of lime is so constantly low. The reaction between organic matter and the sulphates present in sea water (to which we have referred) tends also to keep the amount of carbonate of lime in solution at about one-half (0.12 grms.) of what it might contain (0.28 grms.) per litre. This peculiarity of sea water in taking up a large amount of amorphous carbonate of lime and throwing it out in crystalline form, accounts for the filling up of the interstices of massive coral with crystalline carbonate in coral

[1] *Loc. cit.*, pp. 82–83.
[2] *Loc. cit.*, p. 84.

islands and other calcareous formations, so that all traces may ultimately be lost of the original organic structures.[1]

The authors explain the disappearance of shells and lime deposits in the greater depths of the ocean by their being dissolved by the carbonic acid in the water which is present in larger quantity at great depths and also is produced by the decomposition of the animal matter of the shell and of the various organisms living in the water and on the bottom. They conclude that:

"On the whole, however, the quantity of carbonate of lime that is secreted by animals must exceed what is re-dissolved by the action of sea water, and at the present time there is a vast accumulation of the carbonate of lime going on in the ocean. It has been the same in the past, for with a few insignificant exceptions all the carbonate of lime in the geological series of rocks has been secreted from sea water, and owes its origin to organisms in the same way as the carbon of the carboniferous formations; the extent of these deposits appears to have increased from the earliest down to the present geological period."[2]

In their report on deep-sea deposits, collected by the Challenger expedition, Messrs. Murray and Renard state that the chemical products formed *in situ* on the floor of the ocean nearly all originate in a sort of broth or ooze, in which the sea water is but slowly renewed. Many of them appear to be formed at the surface of the deposit,—at the line separating the ooze from the superincumbent water, where oxidation takes place. In the deeper layers of the deposit a reduction of the higher oxides frequently occurs, and at the surface of the mud or ooze there are many living animals as well as the dead remains of surface plants and animals.[3] They also conclude that practically all the carbon of marine organisms must ultimately be resolved into carbonic acid, the quantity of that acid produced in this way must be enormous, and cannot but exert a great solvent action not only on the dead calcareous structure, but also on the minerals in the muds on the floor of the ocean.[4] Of the effect of this destructive action, they say: "In all cases, however, calcareous structures of all kinds are slowly removed from the bottom of the ocean on the death of the organisms,

[1] *Loc. cit.*, pp. 94–95.
[2] *Loc. cit.*, p. 100.
[3] Report on the Scientific Results of the Voyage of H. M. S. Challenger. Deep-sea Deposits. 1891, p. 337.
[4] *Loc. cit.*, p. 255.

unless rapidly covered up by the accumulating deposits, and in this way protected to a certain extent from the solvent action of the sea-water. It is evident from the Challenger investigations that whole classes of animals with hard calcareous shells and skeletons, remains of which one might suppose would be preserved in modern deposits, are not there represented; although they are now living in immense numbers in the surface waters or on the deposits at the bottom in some regions, yet all traces of them have been removed by solution. A similar removal of calcareous organic structures has undoubtedly taken place in the marine formations of past geologic ages."[1]

From the preceding statements it is evident that initially the greater part of the carbonate of lime is taken from the sea-water by organic agency, but in the working over of this material in the chemical laboratory at the bottom of the sea a considerable portion is taken up by the sea-water as amorphous carbonate of lime and thrown out in the crystalline form to form the matrix of the undissolved shells, etc.

Mr. Bailey Willis has recently studied the question of the deposition of carbonate of lime, and states that "chemists describe two conditions under which bicarbonate of lime may be decomposed into neutral carbonate and carbonic acid. First, by diminution of the tension of the carbonic acid in the atmosphere; second, by agitation of the solution."

"Theoretically either one of three things may occur to the neutral carbonate of lime, if it be thrown out of solution by either one of these processes. The carbonate may be redissolved, deposited as a calcareous mud, or built into organic structures." He studied some recent limestone deposited in the Everglades of southern Florida and found it to be formed of fragments of shells embedded in calcite. He states that "Under the microscope the unaltered structure of the organic fragments is strikingly different from that of the coarse holocrystalline matrix, in which it is apparent that the crystals developed in place. Were this a limestone of some past geologic period it would be concluded, on the evidence of the crystalline texture of some parts of it, that it had been metamorphosed and that the organic remains now visible had escaped

[1] *Loc. cit.*, p. 277. In this connection I wish to ask the student to read Messrs. Murray and Irvine's remarks on pp. 97-99, Proc. Royal Soc. Edinburgh, Vol. 17, 1890.

the process which altered the matrix. But the observed conditions of its formation preclude the hypothesis of secondary crystallization."[1] Apparently the crystalline matrix is one primary product, and the calcareous mud is another, which being precipitated in the solution remains an incoherent sediment.

I think we may accept the conclusion that the deposition of carbonate of lime is by both organic agency and chemical precipitation. It is not necessary to speak of deposition by mechanical methods except in relation to the deposition of chemically derived granules. This probably takes place, and may be a very important factor in the formation of limestones, in seas receiving a large supply of calcium from the land. Calcareous conglomerates do not enter as a prominent deposit in the Cordilleran area.

There is no evidence in the marine geologic formations of this continent that they were deposited in the deep sea; on the contrary, they are unlike such deposits and bear positive evidence of having been laid down in relatively shallow waters. Limestones with ripple-marks and sun-cracks occur, and beds of ripple-marked sandstones alternate with shales and limestones. The more massive limestones, however, appear to have accumulated in deeper water. The conditions in the Cordilleran sea were, I think, more favorable for rapid deposition than in the deep open ocean, but probably not as favorable as about coral reefs and islands. The limestones and often the contained fossils clearly indicate the presence of many of the same conditions of deposition as described by the authors I have quoted. More or less decomposed shells occur in nearly every limestone; and a large proportion of limestones, especially the non-metamorphic marbles, clearly show that they were deposited under the influence of the agencies at work in the laboratory of the sea. Willis states that this occurs in the shallow waters of the Everglades of Florida, and there is no *a priori* reason why it did not occur throughout geologic time; on the contrary, there is no doubt that it did.

*Rate of deposit in former times.* It has frequently been assumed that in the earlier epochs the conditions were more favorable for rapid denudation and in consequence thereof the transportation and deposition of sediment was greater. Professor Prestwich considers[2] that prior to the sedimentary rocks the land surface consisted of

[1] See Mr. Willis' article in Journal of Geology, Chicago, July, August, 1893.
[2] Geology, Vol. 1, 1886, pp. 60–61.

crystalline or igneous rocks subject to rapid decomposition owing to the composition of the atmosphere and to their inherent tendency to decay. They must have yielded to wear and removal with a facility unknown amongst mechanically-formed and detrital strata where erosion operates. He thus accounts for one of the factors that gave the large dimensions and thicknesses of the earlier formations. Mr. Wallace thinks that geological change was probably greater in very remote times,[1] stating that all telluric action increases as we go back into the past time and that all the forces that have brought about geological phenomena were greater.[2]

Dr. Woodward says on the opposite view, that in the earliest geological periods each bed of sand, clay, limestone, etc., had actually to be formed, and that later deposits had the older sedimentary ones to furnish material, and, therefore, the newer deposits were laid down more rapidly.[3] This does not impress me strongly; but from my experience among the Paleozoic rocks I agree with Sir A. Geikie, that "we can see no proof whatever, nor even any evidence which suggests, that on the whole the rate of waste and sedimentation was more rapid during Mesozoic and Paleozoic time than it is to-day.[4]

Professor Huxley, in his presidential address to the Geological Society of London in 1870, treats of the distribution of animals, and says of his hypothesis that it "requires no supposition that the rate of change in organic life has been either greater or less in ancient times than it is now; nor any assumption, either physical or biological, which has not its justification in analogous phenomena of existing nature."[5]

In the Grand Canyon of the Colorado, Arizona, there are 11,950 feet of strata of Algonkian age extending unconformably beneath the Cambrian. There is nothing in this section to indicate that the conditions of deposition were unlike those of the strata of Paleozoic and Mesozoic time. The sandstones, shales, and lime-

[1] Island Life, 2d Ed., 1892, pp. 223–224.

[2] Sir William Thompson (Lord Kelvin) inferred from his investigations upon the cooling of the earth, that the general climate cannot be sensibly affected by conducted heat at any time more than 10,000 years after the commencement of superficial solidification. Treatise on Natural Philosophy, Cambridge, 1883, Vol. 1, pt. 2, p. 478.

Of the degree of the sun's heat we know so little that conjectures in relation to it have little force against the conditions indicated by the sedimentary rocks and their contained organic remains.

[3] Geol. England and Wales, 2d Ed., 1887, p. 23.

[4] Rept. Sixty-second Meeting Brit. Assoc. Adv. Sci., 1892, p. 19.

[5] Quart. Jour. Geol. Soc., Vol. 26, 1870, p. lxiii.

stones are identical in appearance and characteristics with those of the latter epoch. The deposition of sulphate of lime and gypsum occurred abundantly in the upper portions of the series, and salt is collected by the Indians from the deposits formed by the saline waters issuing from the sandstone 8,000 feet below the summit of the series. The sandstones and shales were deposited in thin, even laminæ and layers, and the sun-cracks and ripple-marks give evidence of slow, uniform deposition. In the upper or Chuar terrane, there are 235 feet of limestone. And in one of the layers of limestone, 2,700 feet below the summit of the Chuar terrane, I find abundant evidence of the presence of spiculæ of sponges and of what appear to be worn fragments of some small fossils. There is absolutely nothing to indicate more rapid denudation and corresponding deposition in this early pre-Cambrian series than we find in the Paleozoic, Mesozoic or Cenozoic formations.

### PALEOZOIC SEDIMENTS OF THE CORDILLERAN SEA.

The great sections of sedimentary rocks in Arizona, Nevada, Utah, Montana, and in Alberta, B. A., all bear evidence that the sediments of which they are built up were deposited in a connected and continuous sea that extended from the vicinity of the thirty-fourth parallel, on the south, to the Arctic ocean on the north. Judging from the data now available the width of this sea varies from 300 miles in Nevada to 500 miles on the line of the fortieth parallel, and, with interruptions by mountain ridges, to 250 miles on the forty-ninth parallel. It appears to have narrowed to the north in Alberta and British Columbia. Roughly computed, it covered, south of the fifty-fifth parallel, 400,000 square miles, exclusive of any extension westward into northern central California and southwestern Oregon and to the eastward over the area subsequently covered by the great interior Cretaceous sea. There is also an addition that might be made to allow for the contraction of the area by the later north and south faults and thrusts. Dr. G. M. Dawson estimates that in the Alberta and British Columbia area the width of the zone of Paleozoic rocks has probably been reduced one-half by the folding and faulting, or from 200 to 100 miles.[1] The area assumed for the Cordilleran sea is on this account probably one-half less than it was before the close of the Appalachian revolution.

[1] Bull. Geol. Soc. Am., Vol. 2, 1891, p. 176.

The Wahsatch section, on the eastern side of the area under consideration, has 30,000 feet of strata, of which 10,400 feet are limestone.[1] Further to the west, 250 miles W. S. W., at Eureka, Nevada, there are 30,000 feet of strata in the entire section, and of this amount 19,000 feet are referred to limestone.[2] In the Pahranagat range and vicinity, 200 miles south of the Eureka section,[3] the limestones of the Paleozoic measure over 13,000 feet in a section of 15,500 feet. This section includes only 350 feet of the upper beds of the lower quartzite series, which is upwards of 11,000 feet in thickness in the Schell Creek range of eastern Nevada.[4]

On the eastern side of the area, in Montana, 300 miles north of the Wahsatch section of Utah, the deposit of Paleozoic sediment is less in volume. Dr. A. C. Peale's section gives 3,800 feet of limestone in 5,000 feet of strata.[5] This does not include the 6,000 feet or more of sediments that occur below the fossiliferous Cambrian. I believe that the Paleozoic section will be found to be considerably thicker to the westward, in Idaho. Continuing to the north 450 miles, the sections measured by Mr. R. G. McConnell give 29,000 feet of Paleozoic strata, including 14,000 feet of limestones.[6] In a "Note on the Geological Structure of the Selkirk Range," Dr. Geo. M. Dawson describes a section containing upwards of 40,000 feet of mechanical sediments, which he refers largely to the Cambrian.[7]

The Paleozoic limestones extend to the north, on the line of the eastern Rocky mountains, to the Arctic ocean. In latitude 55° to 60° N. the Devonian limestones are over 2,500 feet in thickness, and there are other still lower Paleozoic rocks that have not yet been studied in detail. The Devonian limestones extend 700 miles in the valley of the Mackenzie, from Great Slave Lake to below Fort Good Hope.[8] No Carboniferous limestones have been described from this region.

Tabulating the sections south from the fifty-fifth parallel and al-

[1] Geol. Expl. Fortieth Parallel, Vol. 1, 1878, pp. 155–156.
[2] Mon. U. S. Geol. Survey, Vol. 20, 1892, p. 178.
[3] *Loc. cit.*, pp. 186–200.
[4] Geol. and Geog. Surveys west of 100th Merid., Vol. 3; Geology, 1875, p. 167.
[5] Author's manuscript.
[6] Geol. and Nat. Hist. Sur. Can.; Ann. Rep., 1886, pp. 17D–30D.
[7] Bull. Geol. Soc. Am., Vol. 2, 1891, p. 168.
[8] Rept. Expl. Yukon and Mackenzie River's Basins, N. W. Terr., Geol. and Nat. Hist. Sur. Canada, Vol. 4 (1888-89), 1890, pp. 13D-18D.

lowing for a great thinning out of the sediments in Idaho and Montana, we obtain an approximate general average of 21,000 feet of strata, of which 6,000 feet are limestone over an area estimated to include 400,000 square miles. Each square mile includes 27,878,400 cubic feet of limestone for each foot in thickness and 167,270,400,000 cubic feet for a thickness of 6,000 feet, which, with an average of 12.5 cubic feet to the ton, gives 13,381.632,000 tons of limestone and impurities per square mile. The result of ten analyses of clear limestones within the central portion of the area gives an average of 76.5 per cent of carbonate of lime.[1] Taking 75 per cent as the proportion of pure carbonate of lime (after deducting 50 per cent to allow for arenaceous and argillaceous material in partings of strata, etc.), there remain 5,018, 112,000 tons per square mile; multiplying this by 400,000 the result gives the number of tons of carbonate of lime that were deposited in what we know of the Cordilleran sea in Paleozoic time,—or 2,007,244,800,000,000 tons, or two billion million tons in round numbers.

The following mode of presentation of the above was suggested by Mr. Willis:

"In order to proceed with a calculation of the period required to form this thickness of 15,000 feet of mechanical sediment plus 6,000 feet of calcareous sediment, it is necessary, first, to compute the cubic volumes of the sediments; second, to estimate the area from which they were derived; and third, to divide the cubic contents of the sediments by this land area. The result thus obtained represents the depth of erosion required to furnish the whole deposit, from which we may estimate the time under different assumptions of the rate of erosion.

But if we express amounts in cubic feet or tons the figures pass all comprehension; therefore to simplify the statement it is well to use a mile-foot as a unit of volume, that is, the volume of 1 mile square and 1 foot thick. (1 mile-foot = .79 kilometer-meter.) This is equal to 223,000 tons, if $12\frac{1}{2}$ cubic feet of limestone equal one ton.

Thus stated, mechanical sediments covering 400,000 square miles and 15,000 feet thick contain 6 billion mile-feet (4,740 million kilometer-meters); and calcareous sediments covering the

[1] Geol. Expl. Fortieth Par., Vol. 2; Mon. U. S. Geol. Survey, Vol. 20.

same area and 6,000 feet thick correspond to 2 billion and 400 million mile-feet (1,896 million kilometer-meters). In the calcareous sediments a liberal allowance of one-half may be made for arenaceous and argillaceous matter in the limestone and partings, and analyses of ten clear limestones within the central part of the area give a little more than 75 per cent of carbonate of lime. Applying these reductions we get 900 million mile-feet (711 million kilometer-meters) of pure carbonate of lime."

### DURATION OF PALEOZOIC TIME IN THE CORDILLERAN AREA.

*Estimates from mechanical sedimentation.* The land area tributary to the Cordilleran sea was larger before the depression of the continent, towards the close of middle Cambrian time, than during subsequent Paleozoic time. It included a portion of the region to the eastward and probably a belt of land extending well towards the Pacific coast of the continental plateau. The interior (Mississippian) region, west of the ninetieth meridian, probably drained into the sea to the south, forming a Cambrian Mississippi river prior to middle Cambrian time. This limits the Cambrian drainage into the Cordilleran sea to an area estimated at 1,600,000 square miles. The average thickness of mechanical sediments deposited before upper Cambrian time is estimated at from 10,000 to 15,000 feet. Taking the minimum of 10,000 feet and the assumed drainage area of 1,600,000 square miles and the rate of denudation at one foot in 1,000 years, it would have required 2,500,000 years to carry to the sea and distribute the 10,000 feet of sediment. This means the deposition of .048 of an inch per year, which is very small if the supposed conditions of denudation and transportation were as favorable as the character and mode of occurrence of the sediments indicate. If one-fourth of an inch per year is assumed as the rate of deposition, the 10,000 feet of sediment would have accumulated in 480,000 years, or in round numbers in 500,000 years, which increases the rate of denudation to one foot in 200 years.[1]

[1] By Mr. Willis' method (ante, p. 157) the mechanical sediments of the Paleozoic age for the area under consideration correspond to six billion mile-feet. Of this total the greater part, namely, two-thirds or four billion mile-feet, are of Cambrian age. Dividing this volume by the land area just given, 1,600,000 square miles, we get 2,500 feet as the depth of erosion during the formation of the Cambrian mechanical sediments. Assuming differing rates of erosion we may obtain times differing as follows:

In dealing with the post-middle Cambrian mechanical sediments we have a somewhat different problem, but, as a whole, rapid deposition is indicated. For instance, the Eureka quartzite of the upper Ordovician is a bed of sandstone, varying from 200 to 400 feet in thickness, distributed over a wide area, perhaps 50,000 square miles. It is made almost entirely of a white, clean sand that was deposited in so short an interval that the Trenton fauna in the limestone beneath it and in the limestones above it is essentially the same. The sand appears to have been swept rapidly into the sea and distributed by strong currents. The same is true of the 3,000 feet of the lower Carboniferous sand and the 2,000 feet in the upper portion of the Carboniferous, while the shales of the upper Devonian accumulated more slowly. In this connection we must bear in mind that during the long periods in which the calcareous sediments forming the limestones were being deposited, the tributary land areas were in all probability base-levels of erosion, and chemical denudation was preparing a great supply of mechanical material that, on the raising of the land, was rapidly swept into the sea and distributed. In this manner the time period of actual mechanical denudation was materially shortened, yet, on account of the manifestly slower deposition of the Devonian shales, the rate of denudation should be assumed as less than during Cambrian time.

In post-Cambrian time the area of the land surface was materially reduced by subsidence, which did not, however, greatly extend the Cordilleran sea, and it may fairly be estimated at 600,000 square miles. The depth of mechanical sediments already estimated is 5,000 feet and their volume 2,000,000,000 mile-feet. Dividing the

CAMBRIAN MECHANICAL SEDIMENTS.

| Rate of erosion over land area of 1,600,000 square miles. | Time in years for erosion of 2,500 feet. | Rate of deposition over sea area of 400,000 square miles for strata 10,000 feet thick. |
|---|---|---|
| 1 foot in 3,000 years. | 7,500,000 | 1 foot in 750 years, or .016 inch per annum. |
| 1 foot in 1,000 years. | 2,500,000 | 1 foot in 250 years, or .048 inch per annum. |
| 1 foot in 200 years. | 500,000 | 1 foot in 50 years, or .24 inch per annum. |

In view of the evidence of rapid accumulation contained in the strata themselves the most rapid rate of deposition here stated, namely, .24 inch per annum, is considered as the most probable.

volume by the area of erosion we get 3,300 feet as the depth of erosion required.

Again applying different rates of erosion with allowance for slow progress of degradation during Devonian time, we have:

POST-CAMBRIAN MECHANICAL SEDIMENTS.

| Rate of erosion over land area of 600,000 square miles. | Time required for removal of 3,300 feet. | Rate of deposition in sea of 400,000 square miles, for 5,000 feet of strata. |
|---|---|---|
| 1 foot in 3,000 years. | 9,900,000 years. | 1 foot in 1980 years, or .006 inch per annum. |
| 1 foot in 1,000 years. | 3,300,000 years. | 1 foot in 660 years, or .018 inch per annum. |
| 1 foot in 200 years. | 660,000 years. | 1 foot in 132 years, or .09 inch per annum. |

The rate of one foot in 200 years is assumed as the most probable and 660,000 years as the time required for the removal and deposition of the 5,000 feet of post-Cambrian mechanical sediments.

There is one factor that may need to be taken into consideration in estimating the time duration of the deposition of the mechanical sediments of the Cambrian and pre-Cambrian of the northern portion of the Cordilleran sea that would materially lengthen the period. Dr. George M. Dawson describes the Nisconlith series, especially in the Selkirk range of British Columbia, as composed of "blackish argillite-schists and phyllites, generally calcareous, with some beds of limestone and quartzite, 15,000 feet."[1] It is correlated with the Bow River series which contains, in the upper portion, the lower Cambrian fauna. The presence of these calcareous beds indicates a slower rate of deposition than we have estimated for the lower portion of the Cambrian series over the greater part of the Cordilleran sea; but as yet the correlation with the sediments of the Cordilleran sea is not sufficiently well established to warrant our allowing a greater time period to the Cambrian on this account.

*Estimates from chemical sedimentation.* We have estimated that the Paleozoic sediments of the Cordilleran sea contain 2,007,244,-800 million tons (900 million mile-feet) of carbonate of lime, which was derived by organic or chemical agencies from the sea water to which it was contributed by the land. If oceanic circulation could be excluded from the problem we might proceed directly to estimate the time required to obtain this amount of lime from the land

[1] Bull. Geol. Soc. Am., Vol. 2, 1891, p. 168.

area tributary to the Cordilleran sea. It may be well to make such an estimate on the basis that the area of denudation tributary to the Cordilleran sea in post-middle Cambrian time had 600,000 square miles from which 30,000,000 tons of carbonate of lime and 12,000,000 tons of sulphate of lime were derived per annum;[1] if we assume T. Mellard Reade's rate of erosion—of 50 tons of carbonate of lime and 20 tons of sulphate of lime per square mile per annum. If all of the 42,000,000 tons (equal to 18.8 mile-feet) per annum were deposited within the limits of the Cordilleran sea, it would have taken 47,790,000 years for the accumulation of the carbonate of lime now estimated to have been deposited in the Cordilleran sea. Such a result is manifestly a maximum, based on the consideration of one set of phenomena. In addition, however, to this supply of calcium, the geographic conditions appear to have been favorable to the free circulation of oceanic currents through the Cordilleran sea, and the temperature was favorable to extensive evaporation and to the development of organic life, as shown by the occurrence of corals in the middle and upper portions of the Paleozoic, from the Mackenzie River basin on the north to southern Nevada on the south. These conditions would reduce the time necessary for the deposition of the carbonate of lime.

Ocean water of the present time contains in solution 151,025,000 tons of solid matter per cubic mile which is divided among various salts. A comparison of the matter in the sea and river water shows that the sea contains 3.85 parts of magnesium to one of calcium, and river water contains three parts of calcium to one of magnesium. The silica and alumina of the river water disappear in sea water, while the sodium is accumulated. It is from these considerations and the fact that limestones are so largely formed of carbonate of lime that I have taken the latter as a basis for estimates upon the rate of chemical sedimentation, an allowance being made for the presence of silica, alumina and magnesium in the limestones.

*Rate of deposition in recent deposits.* Of the rate of deposition in recent deposits Messrs. Murray and Renard state, in their report on the deep-sea deposits, that: "It must be admitted that at the present we have no definite knowledge as to the absolute rate of accumulation of any deep-sea deposit, although we have some in-

[1] Messrs. Murray and Renard consider that organisms have the power of secreting the carbonate of lime from the sulphate of lime contained in the sea water by chemical reaction. For an account of the chemical action that takes place in the sea-water see report of the Deep-sea Deposits of the Challenger Expedition.

formation and some indications as to the relative rate of accumulation of the different types of deposits among themselves. The most rapid accumulation appears to take place in the Terrigenous Deposits, and especially in the Blue Muds, not far removed from the embouchures of large rivers. Here no great time would seem to have elapsed since the deposit was formed, so far at least as the materials collected by the dredge, trawl, and sounding tube are concerned.

Around some coral reefs the accumulation must be rapid, for although pelagic species with calcareous shells may be numerous in the surface waters, it is often impossible to detect more than an occasional pelagic shell among the other calcareous débris of the deposits.

"The Pelagic Deposits as a whole, having regard to the nature and condition of their organic and mineralogical constituents, evidently accumulate at a much slower rate than the Terrigenous Deposits, in which the materials washed down from the land play so large a part. The Pteropod and Globigerina oozes of the tropical regions, being chiefly made up of the calcareous shells of a much larger number of tropical species, must necessarily accumulate at a greater rate than the Globigerina oozes in extra-tropical areas or other organic oozes. Diatom ooze, being composed of both calcareous and silicious organisms, has, again, a more rapid rate of deposition than the Radiolarian ooze, while in a red clay there is a minimum rate of growth."[1]

Prof. James D. Dana estimates that the rate of increase of coral reef limestone formations, where all is most favorable, does not exceed perhaps a sixteenth of an inch a year, or five feet in a thousand years. Of this he says: "And yet such limestones probably form at a more rapid rate than those made of shells."[2]

Messrs. Murray and Irvine, in their valuable paper on coral reefs and other carbonate of lime formations in modern seas, calculate the total amount of calcium in the whole ocean to be 628,340,000 million tons; also they estimate that 925,866,500 tons of calcium are carried into the ocean from all the rivers of the globe annually. At this rate it would take 680,000 years for the river drainage from the land to carry down an amount of calcium

[1] Report on the scientific results of the voyage of H. M. S. Challenger; Deep-sea Deposits. 1891, pp. 411–412.

[2] Corals and Coral Islands, 3d Ed., 1890, pp. 396–397.

equal to that at present existing in solution in the whole ocean. They say further: "Again, taking the 'Challenger' deposits as a guide, the amount of calcium in these deposits, if they be 22 feet thick, is equal to the total amount of calcium in solution in the whole ocean at the present time. It follows from this that if the salinity of the ocean has remained the same as at the present during the whole of this period, then it has taken 680,000 years for the deposits of the above thickness, or containing calcium in amount equal to that at present in solution in the ocean, to have accumulated on the floor of the ocean."[1] According to this calculation the mean rate of accumulation over existing oceanic areas is $\frac{22}{680,000}$ or .000032 feet per annum.

*Was deposition of chemical sediments more rapid during Paleozoic time?* It has been claimed that the quantity of lime poured into the ocean in earlier times was greater than during the later epochs of geological history,—this arising from the more rapid disintegration of the Archean, crystalline, and volcanic rocks. It is undoubtedly a fact that the ocean was stocked in Archean and Algonkian times with matter in solution that produced salinity, but we have no evidence from chemical precipitation that more calcium was poured into it than could be retained in solution. The Laurentian limestones are crystalline, but, as has been shown, this texture is consistent with either chemical or organic origin. The unaltered limestones of the Algonkian rocks of the Colorado Canyon section show traces of life in thin sections, and they may, to a great extent, be of organic origin. There is no evidence in the texture, bedding, or composition of ancient limestones to indicate that they were deposited under conditions of salinity or of supply differing materially from those of the present, and I do not find that we have reason to believe that the deposition of the carbonate of lime was more rapid in the Paleozoic than during the Mesozoic and Cenozoic times, even though the supply from the land may have been greater. Where the conditions were favorable for the deposition of lime, as in the Cretaceous sea of northern Mexico, we find evidence of an immense accumulation of calcareous sediments. Of the amount of calcareous deposits in the seas outside of the continental areas that are not open to our inspection, we know nothing; but judging from the deposition that is going on to-day in the great oceans, the accumulation of calcareous sediments

[1] Proc. Royal Soc. Edinburgh, Vol. 17, 1890. p. 101.

has gone on in the past as steadily and uninterruptedly as at present, subject to varying conditions of temperature, life, depth of water, etc.

*Area of deposition in Paleozoic time.* We have no proof that the salinity of the sea or the amount of calcium contained in it has varied from age to age since Algonkian time. If it has not, all of the calcium poured into the ocean during 2,000,000 years would have about equalled the amount now contained in the limestones of the Cordilleran area. We have, however, to account for the calcium deposited in the interior Mississippian sea and the seas over other portions of this continent and other continental areas and on portions of the floor of the ocean that are not now accessible for observation. It is also to be considered that the land areas subject to denudation in Paleozoic time were, in all probability, of no larger extent than at the present time.

The area of dry land to-day is estimated to be 55,000,000 square miles and of oceans 137,200,000 square miles.[1]

Mr. T. Mellard Reade estimates the area of the Paleozoic formations of Europe at 645,600 square miles in the total area of 3,720,500 square miles. His estimate of the Paleozoic area is of that which is exposed at the present time and does not include that which is concealed beneath other formations. I think it will be a minimum estimate to consider that an equal area is covered by the later formations which, with that exposed, would give in round numbers 1,290,000 square miles, or one-third of the land area of Europe. In North America nearly one-half of the total area was covered by the Paleozoic sea; in South America it was considerably less; and we know too little of the Asiatic and African continents to place any estimate upon their Paleozoic areas. I think, however, if we take one-fourth of the present land area as the territory covered by the Paleozoic seas we shall be considerably within the actual amount, even if we add to the surface of the continents the margins of the continental platforms now beneath the sea. Deducting the one-fourth from the total land area, there remain 41,250,000 square miles as the land area undergoing denudation during Paleozoic time. It may be claimed that large areas in the archipelago region of the Pacific and in the Arctic ocean may have been land areas at that time. To meet this, 8,750,000 square miles may be added to the 41,250,000, giving a total of 50,000,000 square miles as the land area of Paleozoic time.

[1] Dr. John Murray, Scottish Geog. Mag., Vol. 4, 1888, p. 40.

The estimated areas of the various deep-sea deposits of to-day, containing a large percentage of the carbonate of lime, are as follows: Globigerina ooze, 49,520,000 square miles, mean percentage of carbonate of lime 64.53; Pteropod ooze, 400,000 square miles, percentage of carbonate of lime 79.26; coral mud and sand, 2,556,000 square miles, mean percentage of carbonate of lime 86.41. In addition to this, Diatom ooze covers an area of 10,880,000 square miles, with 22.96 percentage of carbonate of lime; and the mean percentage of carbonate of lime in the Blue Mud and other Terrigenous Deposits that covers 16,050,000 square miles is 19.20. If we consider only those deposits containing over 64 per cent. of carbonate of lime, we have 52,500,000 square miles, over which there is at the present time a deposition of the carbonate of lime being made. We have roughly estimated that in Paleozoic time the area of the Paleozoic sea, in which deposits were being accumulated, was over 13,000,000 square miles. It does not appear that there is any good reason to suspect that the area of deposition of the carbonate of lime in the open ocean during Paleozoic time was not fully equal to that of the present time. Adding this area of 52,500,000 to the 13,750,000, we have over 66,000,000 square miles as the probable area in which calcium was being deposited in Paleozoic time.

*Conditions favorable for a rapid deposition of the carbonate of lime.* The conditions most favorable for the rapid accumulation or deposition of the carbonate of lime through organic or chemical agency are warm water and a constant supply of water through circulation by currents; this is shown by the immense abundance of life where the margin of the continental plateau is touched by the Gulf Stream. Another favorable condition is the supply of carbonate of lime by river water directly into the ocean in the vicinity where the deposition of lime is going on either through organic or inorganic agencies. This is well illustrated by the conditions produced by the Gulf Stream. The oceanic currents, passing along the northeastern coast of South America, sweep the waters of the Amazon through the Caribbean sea into the Gulf of Mexico, where they meet the vast volume of water coming from the Mississippi. These are poured out through the narrow straits between Florida and Cuba, and carried northward over the sloping margin of the continental plateau. Under such favorable conditions the deposit must be much greater than in areas where there

is little circulation and the supply of calcium is limited to the average which is contained in sea water. If to the preceding there be added extensive evaporation within a partially enclosed sea, the rate of deposition of matter in solution will be largely increased.

*Estimates from deposition of calcium derived from Cordilleran sea and the outer ocean, and from the deposition of mechanical sediments.* The area over which calcareous deposition was going on during Paleozoic time we have estimated at 66,000,000 square miles, which includes the areas of the seas over the continental platforms and those of the surrounding oceans. As the conditions appear to have been more favorable for the deposition of lime in the Cordilleran and Appalachian seas, we will assume that it was four times that of the open ocean.[1] With a land area of 50,000,000 square miles (ante, p. 164) and a rate of chemical denudation of 70 tons per square mile per annum, the total calcium contributed to the ocean per year during Paleozoic time would be 3,500 million tons, or 3.78 times as much as that estimated per annum at the present time, which is 925,866,500 tons. This would have provided 50.7 tons for deposition per annum per square mile in the 65,000,000 square miles of ocean and seas and 202.8 tons for deposition per annum per square mile in the 400,000 square miles of the Cordilleran and 600,000 square miles of similar seas. On this basis 81,120,000 tons (36.4 mile-feet) were contributed per annum from the ocean water to the deposit in the Cordilleran sea, adding to this the 42,000,000 tons (18.8 mile feet) contributed per annum by the denudation of the surrounding area to the Cordilleran sea, we have 128,120,000 tons (55.2 mile-feet) as the amount available for deposit per annum in the Cordilleran sea. At this rate it would have required 16,300,000 years to have deposited the 2,007,244,800 million tons (900 million mile-feet) of *calcium* in the Cordilleran sea; adding to this the 1,200,000 years estimated for the deposition of the mechanical sediments, we have a total of 17,500,000 years as the duration of Paleozoic time.

[1] Under the reduction of 50 per cent. for the interbedded and intermingled mechanical sediments and 25 per cent for other material than calcium deposited from solution, the apparent amount of calcium deposited in the Cordilleran sea was greatly reduced. If this same ratio of reduction is applied to other Paleozoic limestone areas I doubt if over 1,000,000 square miles will be found to contain as large an average amount of calcium per square mile as the Cordilleran area. On this account 1,000,000 square miles is the area taken for the greater rate of deposition of calcium during Paleozoic time.

In reviewing the preceding estimates we must consider that throughout I have increased the various factors above those usually accepted; thus, for mechanical sedimentation, the erosion of one foot in 200 years is used. If the usually accepted average of one foot in 3,000 years is taken the time period must be increased fifteen fold (21,000,000 years), or the area of denudation from 1,600,000 square miles to 24,000,000,—or three times the present area of the North American continent.

In the estimate for the amount of chemical denudation the largest average is taken—70 tons of calcium per square mile per annum—and the assumption made that all calcium derived from the adjoining drainage area was deposited within the Cordilleran sea. Again, the total supply provided per annum to ocean waters of Paleozoic time is taken as 3.78 times greater than the amount annually contributed to ocean waters to-day; of this, four times as much is assumed to have been taken out per annum per square mile in the Cordilleran and 600,000 square miles of similar seas as was taken by the remaining area in which calcium was being deposited.

The area of the Cordilleran sea is given as 400,000 square miles, but it was probably 600,000, if not much more. It may be claimed that the area tributary to the Cordilleran sea was greater than I have estimated. The evidence, such as it is, is against such a view. As a whole I think the estimate of 17,500,000 years for the duration of Paleozoic time in the Cordilleran area is below the minimum rather than above it.

If the estimated rate of the deposition of coral limestones—five feet in 1,000 years—given by Prof. James D. Dana is correct, the 19,000 feet of Paleozoic limestone in central Nevada would have required 3,800,000 years to have accumulated under the most favorable local conditions surrounding a coral reef. With the exception of large deposits of corals in Devonian rocks no appearance of a coral reef is recorded in the Cordilleran area.

## TIME-RATIOS OF GEOLOGIC PERIODS.

The time-ratio adopted by Prof. James D. Dana for the Paleozoic, Mesozoic, and Cenozoic periods is 12, 3 and 1, respectively.[1] Prof. Henry S. Williams applies the term *geochronology*, giving the standard time-unit used, the name *geochrone*. The geochrone

[1] Manual of Geology, 1875, p. 586.

used by him in obtaining a standard scale of geochronology is the period represented by the Eocene. His time-scale gives 15 for the Paleozoic; 3 for the Mesozoic, and 1 for the Cenozoic, including the Quaternary and the Recent.[1]

The Rev. Samuel Haughton obtained the following time-ratios from the maximum thickness of strata as they occur in Europe:

SCALE OF GEOLOGICAL TIME.

| PERIOD. | From theory of cooling globe. | From maximum thickness of strata. |
|---|---|---|
| Azoic | 33.0 per cent. | 34.3 per cent. |
| Paleozoic | 41.0 " " | 42.5 " " |
| Neozoic | 26.0 " " | 23.2 " " |
| Total | 100.0 " " | 100.0 " " |

He draws from this the principle:—"*The proper relative measure of geological periods is the maximum thickness of the strata formed during those periods.*"[2]

In considering the time-ratios for the Paleozoic, Mesozoic and Cenozoic rocks of the North American continent, as given by Dana and Williams, I think that a too small proportion has been given to the Mesozoic and Cenozoic. In the Mesozoic of the western-central area occur the coal deposits of the Laramie series and the great development of limestones (from 10,000 to 20,000 feet) in the Cretaceous of Mexico. The limits of this paper do not permit of a discussion of the available data bearing upon geologic time-ratios; but from a comparison of the Paleozoic, Mesozoic and Cenozoic strata and the geologic phenomena accompanying their deposition, I would increase the comparative length of the Mesozoic and Cenozoic periods so that the time-ratios would be: Paleozoic, 12; Mesozoic, 5; Cenozoic, including Pleistocene, 2.

### DURATION OF POST-ARCHEAN GEOLOGIC TIME.

Taking as a basis 17,500,000 years for Paleozoic time and the time-ratios, 12, 5 and 2, for Paleozoic, Mesozoic and Cenozoic (including Pleistocene), respectively, the Mesozoic is given a time duration of 7,240,000 years; the Cenozoic, of 2,900,000 years; and

[1] Journal of Geology, Chicago, Vol. 1, 1893, pp. 294-295.
[2] Nature, Vol. 18, 1878. p. 2, 268.

the entire series of fossiliferous sedimentary rocks, of 27,650,000 years. To this there is to be added the period in which all of the sediments were deposited between the basal crystalline Archean complex and the base of the Paleozoic. Notwithstanding the immense accumulation of mechanical sediments in this Algonkian time, with their unconformities and the great differentiation of life at the beginning of Paleozoic time, I am not willing with our present information to assign a greater time period than that of the Paleozoic,—or 17,500,000 years. Even this seems excessive. Adding to it the time period of the fossiliferous sedimentary rocks, the result is 45,150,000 years for post Archean time. Of the duration of Archean or pre-Algonkian time I have no estimate based on a study of Archean strata to offer. If we assume Haughton's estimate of 33 per cent for the Azoic period and 67 per cent for the sedimentary rocks, Archean time would be represented by the period of 22,250,000 years. In estimating for the Archean, Haughton included a large series of strata that are now placed in the Algonkian of the Proterozoic of the U. S. Geological Survey; and I think that his estimate is more than one-half too large; if so, ten million years would be a fair estimate, or rather conjecture for Archean time.

| PERIOD. | TIME DURATION. |
|---|---|
| Cenozoic, including Pleistocene................ | 2,900,000 years. |
| Mesozoic................ ...................... | 7,240,000 " |
| Paleozoic........................................ | 17,500,000 " |
| Algonkian........................................ | 17,500,000 " |
| Archean........................................ | 10,000,000(?) " |

It is easy to vary these results by assuming different values for area and rate of denudation, the rate of deposition of carbonate of lime, etc.; but there remains after each attempt I have made that was based on any reliable facts of thickness, extent and character of strata, a result that does not pass below 25 to 30 million years as a minimum and 60 to 70 million years as a maximum for post-Archean geologic time. I have not referred to the rate of development of life, as that is virtually controlled by conditions of environment.

In conclusion, geologic time is of great but not of indefinite duration. I believe that it can be measured by tens of millions, but not by single millions or hundreds of millions of years.

# PAPERS READ.

USE OF THE NAME "CATSKILL." By Prof. JOHN J. STEVENSON, University of the City of New York, N. Y.

[ABSTRACT.]

A RECENT writer has urged the propriety of discontinuing the use of "Catskill" as a term to distinguish a subordinate group and of applying it to the whole series of rocks between the Hamilton below and the Lower Carboniferous above.

This term cannot be discarded as the name of a subordinate group, for the Catskill group of Vanuxem is thoroughly well-defined below, and has its own peculiar distinction. The term cannot be applied to the whole Upper Devonian with propriety, for that would be to apply a geographical term representing conditions which prevailed over an extended area during only the later portion of the period; and even then over only a small portion of the Upper Devonian area, a comparatively narrow strip along the Appalachian outcrop at the east.

[Printed in American Journal of Science.]

CHARACTER OF FOLDS IN THE MARQUETTE IRON DISTRICT. By Prof. C. R. VAN HISE, Madison, Wis.

[ABSTRACT.]

THE dynamic movements which have affected the Marquette district have not produced major faulting. The only faults found are small, being along dykes, intrusive masses, or in places where the rocks are very brittle. A north and south section across the central part of the district shows a great synclinorium. The north side has been pushed under from that direction, producing overturned minor folds. The south side has been pushed under from that direction, also producing overturned minor folds. Upon the whole in passing from the outer borders toward the center of the trough, one passes to higher formations. The folding resembles the fan structure of the Alps, but there is the great difference that the major fold is a synclinorium rather than an anticlinorium. This kind of fold may be called the Marquette type.

REMARKS ON THE GENUS ARTHROPHYCUS, HALL. By JOSEPH F. JAMES, M.Sc., Washington, D. C.

[ABSTRACT.]

*Arthrophycus harlani* and *Harlania halli* are two names applied to the same fossil form. Both were used in the same year, 1852, and there has been some dispute as to which should have priority. The fossil under consideration was described, but not named, by Eaton, in 1820, as occurring in the Red Sandstone of the Niagara River, now known as the Medina. In 1831, Harlan described the species from Pennsylvania under the name of *Fucoides alleghaniensis* and in 1832, a second species as *Fucoides brongniartii*. These are both evidently the same; but the latter should not be confounded with the quite different *F. brongniartii* of Mantell, described in 1833. In 1838, Conrad, for no assigned reason, changed the name to *Fucoides harlani*, and when the generic name was changed by Hall, in 1852, to *Arthrophycus*, he used the specific name *harlani*. Goeppert, in 1852, proposed to call the fossil *Harlania halli*. While the date on the title page of the volumes containing these two descriptions is the same. Hall in his introductory remarks (Palæontology of New York, Vol. II) says that the first pages of the volume were in print in 1849, and that the delay in engraving the plates prevented the appearance of the volume until 1852. That the volume was regarded as printed in 1851 by Hall himself, seems evidenced by several references to it by him in 1851, in Part 2 of Foster and Whitney's "Report of the Lake Superior Land District."

In respect to the specific name, since *alleghaniensis* was the first one proposed, it is believed that it should take precedence over Conrad's name, *harlani*, of 1838. The fossil should therefore be known as *Arthrophycus alleghaniensis* (Harlan), Hall.

[Printed in full in *Journal of Cincinnati Society of Natural History*, Vol. XVI, pp. 52–86.]

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ON THE VALUE OF SUPPOSED-ALGÆ AS GEOLOGICAL GUIDES. By JOSEPH F. JAMES, M.Sc., Washington, D. C.

[ABSTRACT.]

THIS paper may be summed up as follows:

(1) The presumptive evidence of the presence of algæ in early geological time is good, but there is not good evidence that the greater part of species of so-called algæ are really such; and any deductions based upon the number of species and genera recorded by authors, are defective. (2) The great similarity of forms like *Scolithus*, having a very great time-range, is against their use as geological guides. (3) The poor state of preservation and the great variability they present are also arguments unfavorable to their use. (4) Unless the deductions made are corroborated by other evidence than that afforded by presumed fossil algæ, they cannot be considered as having any great value. This other

evidence must come from the presence of undoubted organized forms, or from stratigraphical evidence which cannot be gainsaid.

[Printed in full in *American Geologist*, Vol. XIII, Feb., 1894, pp. 95–101.]

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STUDIES IN PROBLEMATIC ORGANISMS: THE GENUS FUCOIDES. By JOSEPH F. JAMES, M.Sc., Washington, D. C.

[ABSTRACT.]

IN this paper it is proposed to restore the genus *Fucoides*, the type species, *strictus*, being limited to it. It is also proposed to use the name *Gigartinites* for the species placed by Brongniart in the section of this name. These species have been generally placed with *Chondrites*, but this, like *Fucoides*, is a composite genus and will eventually be broken up and distributed among other genera. A list of the species, referred to the genus by various authors at different times, is given at the end of the paper, with a reference to the genus in which each is placed at the present time.

[Printed in Jour. Cin. Soc. Nat. Hist., Vol. XVI, pp. 62–81, pl. 3.]

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NOTE ON FURTHER OBSERVATIONS OF TEMPERATURE IN THE DEEP WELL AT WHEELING, W. VA. By WILLIAM HALLOCK, Columbia College, New York, N. Y., July, 1893.

[ABSTRACT.]

WHEN the observations of 1891 were finished, an oak plug was driven into the top of the casing and thus the hole protected. In July of this year the hole was opened and it was found full of fresh water to within forty feet of the top, having leaked full in something less than two years. Those who should know have no doubt that this water has entered at the lower end of the inner casing, *i. e.*, at 1,570 feet below the surface. This water can be easily bailed out, and will be when drilling is recommenced.

I was very desirous to obtain a series of temperatures with the water in the well, to discover the extent to which its circulation would or does affect the distribution of temperature in the hole.

The ordinary signal service mercurial maximum thermometers were used, inverted, as in 1891. They were enclosed in a heavy, sealed glass tube to protect them from the pressure of the water, and were used in pairs and *all* corrections applied. The two always agreed to within 0° 2 F, except once at 2,669 feet, when one evidently failed to record correctly. Two thermometers were in an iron bucket, three feet long and three inches diameter at the end of the wire, and two were in an open wire frame, two hundred and sixty feet from the end of the wire. The temperatures at depths of one, two and three hundred feet were deter-

mined with other thermometers separately lowered from the top of the well.

The results are given in Table I. The first column contains the depths in feet; the second, the corresponding temperatures in degrees Fahrenheit, as they were found this July. In column three are the temperatures interpolated from Table II, as found in July, 1891. The last column gives the differences, or rise in temperature, in the well in two years.

At 3,200 feet an obstruction occurs, which I was not able to remove with the available tools, and which temporarily prevented an investigation of the lower 1,300 feet of the well. It is not serious — a heavy tool will easily open the well completely. A glance at the fourth column in Table I shows that the temperatures in water to-day are practically identical with those in air two years ago.

TABLE I.

| Depth in feet. | Temperatures in Fahrenheit. | | 1893. mims. 1891. |
|---|---|---|---|
| | 1893. | Interpolated from 1891. | |
| 103 | 52°.58* | ..... | ..... |
| 206 | 53.53 | ..... | ..... |
| 311 | 55.03 | ..... | ..... |
| 1586 | 70.12 | 70°.15 | —°.03 |
| 1921 | 73.95 | 73.82 | +.13 |
| 2055 | 75.28 | 75.42 | —.14 |
| 2276 | 78.13 | 77.93 | +.20 |
| 2396 | 79.54 | 79.45 | 0.09 |
| 2539 | 81.21 | 81.15 | 0.06 |
| 2669 | 83.39† | 82.75 | 0.64† |
| 2793 | 84.56 | 84.41 | 0.15 |
| 2937 | 86.12 | 86.07 | 0,05 |
| 3057 | 87.42 | 87.50 | —.08 |
| 3196 | 89.27 | 89.30 | —.03 |

*Probable temperature at one hundred feet, from other observations, 51°.30.
† Or more probably, 82°.87 and + 0°.12.

TABLE II.

| Depth in feet. | Temperature, Fahr. | Depth in feet. | Temperature, Fahr. |
|---|---|---|---|
| 100 | 51°.30 | 2990 | 86°.60 |
| 1350 | 68.75 | 3125 | 88.40 |
| 1591 | 70.15 | 3232 | 89.75 |
| 1592 | 70.25 | 3375 | 92.10 |
| 1745 | 71.70 | 3482 | 93.60 |
| 1835 | 72.80 | 3625 | 96.10 |
| 1992 | 74.50 | 3730 | 97.55 |
| 2125 | 76.25 | 3875 | 100.05 |
| 2236 | 77.40 | 3980 | 101.75 |
| 2375 | 79.20 | 4125 | 104.10 |
| 2486 | 80.50 | 4200 | 105.55 |
| 2625 | 82.20 | 4375 | 108 40 |
| 2740 | 83.65 | 4462 | 110.15 |
| 2875 | 85.45 | | |

Only once does the difference amount to 0°.2 Fahrenheit, and these differences show no evidence of a warming in the top and cooling in the bottom, as we would naturally expect. It seems to me we are thus compelled to believe that there is not an appreciable circulation even of water in a hole of five inches diameter.

All the results, down to 3,200 feet, give a gradient of 1° Fahrenheit for every 81.5 feet, whereas the last few hundred feet show about 1° Fahrenheit increase for every 60.0 feet. The expenses of these supplementary tests were borne by the United States Geological Survey and it is with the director's permission that this note is published.

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RECENT INVESTIGATIONS IN THE CRETACEOUS FORMATION ON LONG ISLAND, N. Y. By ARTHUR HOLLICK, Columbia College, New York, N. Y.

[ABSTRACT.]

IN a recent communication[1] I gave an account of such facts as were known in regard to the occurrence of cretaceous material on Long Island. I also stated that, theoretically, cretaceous strata and material ought to be present over a much wider area than that in which it was known to occur.

During the present summer an exploration of the north shore of Long Island was made, from Port Jefferson to Glen Cove, with the result that the theoretical predictions were verified in a most satisfactory manner.

Indisputable cretaceous material (mostly palæobotanical) was collected all the way between Glen Cove and Eaton's Neck, and between this locality and Port Jefferson the lithological evidence was satisfactory but not conclusive.

It is hoped to continue the exploration over other areas of the Island in the future.

[The paper was illustrated by maps and specimens.]

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NOTES ON THE NORTHWARD EXTENSION OF THE YELLOW GRAVEL IN NEW JERSEY, STATEN ISLAND, LONG ISLAND AND EASTWARD. By ARTHUR HOLLICK, Columbia College, New York, N. Y.

[ABSTRACT.]

AT the Rochester meeting of the Association specimens were exhibited and an account given of fossil leaves from the Yellow Gravel horizon, near Bridgeton, Cumberland County, N. J.[2]

Some further investigation since then demonstrated that, in order to

[1] "Preliminary Contribution to our Knowledge of the Cretaceous Formation on Long Island and Eastward." Trans. N. Y. Acad. Sci., XII, 222–237.

[2] Proc. A. A. A. S., XLI, 177, 178.

render the work complete, a general examination of the Yellow Gravel northeastward would be advisable With this end in view, considerable field work was done in northern New Jersey, and on Staten Island, Long Island, and Martha's Vineyard, in the course of which some new facts of interest were brought to light.

Stratigraphically and lithologically, the formation is often very difficult to differentiate, but one of the characteristics most relied upon for identification is the presence of palæozoic fossils in the pebbles, which are generally highly silicified, especially towards the north.

These were found in greatest abundance in Middlesex and Monmouth counties, N. J., where the majority of them are Devonian corals, with some molluscs.

While searching this region, an examination was made of some reddish-brown friable sandstone, which overlies the Cretaceous at Atlantic Highlands. This rock is similar to that at Bridgeton, and in it were found fragments of leaf impressions similar to those at Bridgeton, but too indefinite for determination. This is, I believe, the only other locality where such remains have been found, and is therefore worthy of record.

On Staten Island the Yellow Gravel occurs *in situ*, and also mixed with the transported material of the glacial moraine, wherever this has crossed any Yellow Gravel area. Under each of these conditions it may be recognized by means of the characteristic silicified palæozoic fossils.

On Long Island these fossils were found in the gravels eastward to as far as Oyster Bay, in considerable abundance, after which they became less common, and were not found at all east of Eaton's Neck. From this locality to Port Jefferson the identification of the Yellow Gravel was dependent upon its stratigraphic relations. Lithologically, it was evidently granitic. On Martha's Vineyard the gravels were carefully searched for any indications of palæozoic rocks or fossils; but the only result was a single pebble of calcareous rock.

The significance of these facts in the discussion of the origin of the Yellow Gravel is apparent. The pebbles of palæozoic rock become less and less abundant and finally cease, at a point on Long Island where they would naturally cease if derived and transported from outcrops of such rocks to the north and west, while they become quartzose and granitic farther eastward, where we should naturally expect them to be so if derived from the crystalline rocks of the mainland.

The problem which yet needs solution, if this theory of origin and transportation is true, is, why there should be such a paucity of pebbles representing the Triassic rocks over which this line of transportation lies.[1]

[The paper was illustrated by maps and specimens and will be printed in full as a Bulletin of the U. S. Geological Survey.]

[1] Since writing the above, Prof. R. D. Salisbury has informed me of the presence of Triassic material in the Yellow Gravel at localities not before known to me This fact may result in a modification of the concluding paragraph.

AMOUNT OF GLACIAL EROSION IN THE FINGER LAKE REGION OF NEW YORK. By D. F. LINCOLN, M.D., Geneva, N. Y.

[ABSTRACT.]

EXPOSURES of rock are frequent. Running parallel to Lake Ontario, the Clinton, the Niagara shale and limestone are exposed in almost every stream which crosses them to the north (Hall, "N. Y. Reports," 1843). The Salina is "deeply excavated and occupies a depression;" but yields exposures at Lyons and numerous points farther west (Hall). "It is much exposed near its southern border and often quarried." The Niagara limestone does not form a distinct escarpment, though often rising in hillocks thinly covered with soil.

In the lake district proper, the Corniferous is often quarried within two to ten feet of the surface. It forms a low escarpment in places. The Tully limestone forms a partial escarpment amid the Devonian shales. The latter are cut to great depths by the lakes and their tributary gullies.

Large tracts of level rocks, covered with a foot or a very few feet of soil, occur in various places — Niagara, at Sodus; Corniferous, west of Geneva; Hamilton, at Cazenovia (upland); Chemung, in high lands to the south.

A few large hills and ridges of soft shale, very thinly covered, are found in the latitude of Geneva and Aurora. There are large tracts of rolling upland to the south; where the plow often reaches rock. The steep slopes within a mile of the lake shores often have but a foot or two of soil (Auburn), and the cliffs along the large lakes (Seneca, Cayuga), for great distances correspond to this, though they not infrequently display a covering of ten to thirty feet of till.

The heaviest ascertained drift is that filling the north end of Seneca Lake, found by borings to be 202, 212, and 240 (?) feet deep close to the lake. Other buried channels, owing to the lack of borings, have not been indicated.

On the whole, it is probable that, in the lake region proper, two-fifths of the surface have one to five feet of drift, two-fifths have five to thirty feet, and one-fifth has between thirty and one hundred. A greater thickness than one hundred feet is seldom presumable. The terminal moraine is excluded from this statement. In the drumlin region there are great variations, the depth being great along the Ontario shore, apparently great along the Clyde River, and small along the Clinton and Niagara lines of outcrop. The mass of the drumlins is equivalent to a layer of, perhaps, ten feet.

*Origin of Lake Basins.*—The valley of Seneca Lake is undoubtedly terminated by a rock barrier to the north, since repeated borings at Geneva show the rock, at points probably not far from the true axis, with elevations of two hundred to two hundred and thirty feet above tide, whereas the central-southern portions lie one hundred and seventy-seven feet below tide. The valleys, which form its continuation southward, display rock-bottom at Bloomsburg, below Wilkesbarre, four hundred and fifty feet above tide. To drain the lake northwardly (by buried channels)

would require that the mid-parts of the lake be raised five hundred feet; to drain it southwardly, 1,000 feet. This is suggestive of flexure of the earth's crust subsequent to the formation of the valleys. In accord with this is the known northward depression of the bed-rock of the Susquehanna valley, between Bloomsburg and Wilkesbarre, amounting to one hundred and ten feet.

Glacial erosion seems to have acted much more powerfully on the valleys of the Finger Lakes than on the high lands between. The former are remarkable for great smoothness of their sides and the want of side valleys. Along Seneca Lake there is but one such (at Dresden), and along Cayuga Lake but one (at Ludlowville), in a combined coast line of one hundred and sixty miles. These lake valleys, directly south of the terminal moraine, have side valleys at normal intervals. The supposition that the side valleys have been filled up level with drift along the lake is forbidden by the continuous rock-cliffs with nearly level summits, which line the nearly straight coasts of the southern half of the two lakes named. At a point south of Lodi (Seneca Lake), a slight recession of the coast line, with twenty to fifty feet of drift covering the rock, seems to correspond with the remains of one such valley.

Upon the high land flanking the south end of Seneca Lake, systems of preglacial valleys have survived. They are cut from two hundred to four hundred feet in the hard Chemung shales; their direction is independent of the glacial movement (unlike the lake valleys); they have a moderate grade, and run towards the lake, upon whose banks they open out, and cease to exist at five hundred feet above water-line = 1,100 feet above the present floor of the lake. This occurs at three miles' distance from the axis of the lake. To harmonize the upland valleys with the preglacial lake valley, the latter must have been six (?) hundred feet less deep than it now is. A large part of the deepening is provisionally ascribed by the writer to glaciation.

[This paper was illustrated by a map of the lake region, Section N.–S. from Lake Ontario, Seneca Lake, Upper Susquehanna, and photographs of topography. To be printed in full in "Am. Jour. Science," Feb., 1894.]

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ICE-SHEET ON NEWTONVILLE SANDPLAIN. By F. P. GULLIVER, Norwich, Conn.

[ABSTRACT.]

PAPIER-MACHÉ models — one of the Newtonville sandplain, with its feeding esker, as it exists to-day, and another showing the theoretical condition at the time of its formation—were exhibited with photographs, drawings, and lantern slides. The evidence goes to show that this sandplain was formed in front of an esker-river in the melting ice-sheet, at the head of an arm of the sea, when the water-level was some one hundred and fifty feet higher than at present, near Boston.

[This paper is printed with photographic reproductions of models in the Journal of Geology, Vol. I, pp. 803–812].

CHANGES OF DRAINAGE IN THE ROCK RIVER BASIN IN ILLINOIS. By FRANK LEVERETT, U. S. Geological Survey, Denmark, Ia.

[ABSTRACT.]

THE preglacial Rock River valley has been found to depart from the present course of that stream, a few miles below Rockford, Ill., and passing southward to enter the Green River basin near the head of Inlet swamp. A heavy accumulation of drift so obscures the region farther south, that no data were obtainable as to the exact course, though it is probable that connection was made with the Illinois valley at the great bend near Hennepin.

The change to its present course dates from the early portion of the glacial period, since the new valley was opened to about its present size and depth prior to the formation of the Kettle moraine of the Green Bay Lobe. This moraine crosses the headwaters of Rock River, in Wisconsin and gravels which occupy the new course of the stream were derived from the ice-sheet at the time the moraine was forming, being distinctly traceable up to it as a moraine headed terrace. It furnishes a measure of interglacial erosion, that may be readily compared with the amount of erosion in the same valley subsequent to the deposition of the gravels connected with the Kettle moraine. It is found that the amount of erosion accomplished prior to the formation of this moraine is nearly twice as great as that accomplished by the stream since that time. The erosion, prior to the formation of the Kettle moraine, was mainly in rock strata, while that subsequent to it was almost wholly in gravel and sand. The studies, therefore, sustain the view that the two ice invasions were more widely separated than the whole length of postglacial time, and warrant the use of the term epoch, rather than episode, in the expression of these time relations.

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GRAPHIC COMPARISON OF POST-COLUMBIA AND POST-LAFAYETTE EROSION. By W J MCGEE, Bureau of American Ethnology, Washington, D. C.

[ABSTRACT.]

THE valley of Potomac River, in the vicinity of Washington, illustrates fairly the relative amounts of erosion effected respectively since the period of Columbia deposition and the period of Lafayette deposition. A carefully measured profile connecting remnants of the Lafayette formation and intersecting the post-Columbia gorge of the Potomac shows that here the average amount of post-Lafayette erosion (including that of post-Columbia time) is two hundred and fifty feet, and that it extends over almost the entire land-surface for many hundred square miles; while the post-Columbia erosion averages only about one foot, and is confined to the channels and immediate vicinage of the larger waterways. This difference in amount of erosion shows wide difference in age, and affords a means of interpreting the unconformity between the formations. [The paper was illustrated by maps and diagrams.]

AN ILLUSTRATION OF THE EFFECT OF STAGNANT ICE IN SUSSEX COUNTY, N. J. By Prof. ROLLIN D. SALISBURY, Chicago University, Chicago, Ill.

[ABSTRACT.]

IN some of the river valleys of the broad area known as the Kittatinny valley, there are considerable deposits of stratified drift which, in a general way, take the form of terraces. They depart from normal river terraces in form, in that they frequently lie well up on the slope of the valley, failing to reach its bottom. The bottoms of the valleys are sometimes free from drift and sometimes occupied by till. Another peculiarity of the terraces is the kame-like, or terminal-moraine-like, topography of those slopes of the terraces which front the valley trough. The valleys sometimes have the terraces on one side only, the other being occupied by till. Now and then, spurs of gravel run out from the terraces on the one side or the other of the valley, sometimes nearly crossing the same. Occasionally the terraces on either side widen, so as to meet at intervals in the centre of the valley, leaving basin-like depressions between the opposing terraces, where they fail to meet. These basins give origin to chains of small lakes along the valleys.

It seems clear that in the late stages of the last ice epoch, the ice became stagnant in these valleys, after the intervening ridges had been freed from ice. The drainage then followed the line of junction of the stagnant valley ice, and the rock ridge which confined it. Thus, the position of the terraces, sometimes well up on the valley slope, is explained. The gravel was deposited against the irregular face of the ice, filing and fitting its irregularities. When the ice melted, the terrace face which fitted the ice, retained the irregular kame-like topography, to which the rough face of the ice gave rise. The spurs of gravel, running out from either side toward or beyond the centres of the valleys, are believed to represent the fillings of crevasses in the stagnant ice. The lake basins are thought to represent the sites where large blocks of ice finally became isolated in the dissolution of the ice. It is believed that many chains of lakes, which occur in the valleys of glaciated areas, are to be thus explained.

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A PHASE OF SUPERGLACIAL DRIFT. By Prof. ROLLIN D. SALISBURY, Chicago Univ., Chicago, Ill.

[ABSTRACT.]

THE drift about Madison, both stratified and unstratified, is irregularly covered by a thin mantle (two to four feet) of brown earth, containing bowlders. The bowlders are nearly all of distant origin and mostly angular. It is suggested that the surface earth, in and on which these foreign bowlders occur, is composed mainly of dust which was blown upon the ice, and which was let down upon the surface of the drift when the ice finally disappeared.

TERTIARY AND QUATERNARY STREAM EROSION OF NORTH AMERICA. By
WARREN UPHAM, 36 Newbury St., Somerville, Mass.

[ABSTRACT.]

FROM investigation of the glacial drift and the history of the Glacial period on this continent, and especially from inquiries respecting the causes of the accumulation of the ice-sheets of that time, I have been led to the belief that during the closing part of the Pliocene period, ending the Tertiary era, and in the early part of the Quaternary, North America was greatly uplifted above its present height, to such an altitude that the continent had a high plateau climate with plentiful precipitation of moisture as snow at all seasons of the year. This condition, which I appeal to as the origin of the Ice age, must also have been recorded by very exceptional stream erosion, and if this were not discoverable the view that high altitude of the land was the chief cause of its glaciation would be disproved. The purpose of this paper is to direct attention to geographic features of North America, of the adjoining Arctic archipelago, and of Greenland, which seem explainable by the supposed high late Tertiary and early Quaternary uplift, being thus corroborative of this explanation of the cause of the ice accumulation.

1. Beginning at the southwest and south, we may first look at the area traversed by the Colorado river, where the work of Newberry, Powell, Dutton, Gilbert, and others, show that vast erosion by streams has been in progress during all the Tertiary era, and that a new and more intense cycle of erosion, causing the Colorado to cut the narrow inner and lower part of its cañon 3,000 feet below its previous much wider cliff-enclosed valley, was inaugurated by a new uplift of that country late in Tertiary time, unless it be better regarded as the event separating that from the ensuing Quaternary era.

2. This great epeirogenic movement seems also to have extended through all the southern United States, eastward to the Gulf of Mexico and the Atlantic. In this region it is represented, as I believe, by the Lafayette formation and the extensive erosion between the Lafayette and Columbia epochs. Here we are indebted to Hilgard, McGee, Darton, J. A. Holmes, and others, for the publication of detailed observations, from which I think we must regard both the Lafayette and Columbia formations as the deposits of flooded rivers on the coastal plain, in the Mississippi embayment, and along the greater valleys above their opening into the lowlands. As I look upon this region, the Lafayette loam, sand, and gravel appear to be not a marine but a land formation, derived from stream erosion of the mountains and highlands, bringing down the residuary earth and alluvium of previous subaërial denudation, to be deposited where these streams reached their less steeply descending lower valleys or overspread the coastal plain. The first effect of the epeirogenic uplifting was to overload the streams and lead to the deposition of the Lafayette beds; but as the uplift continued and the whole region rose to a great altitude, the increased gradients of the rivers caused them later to cut

deeply into their former Lafayette deposits, removing a large part of that formation and also cutting into the underlying Tertiary and Cretaceous strata, thereby forming the now partially submerged valleys of Delaware and Chesapeake bays and Albemarle and Pamlico sounds. When the culmination of the uplift brought on the northern ice-sheet, heavy snowfall on the southern uplands and mountains, melting away in spring with copious rains, produced again overloaded river floods and the deposition of the Columbia formation. Under this view the geologic records of the southern states accord with the explanation of the Ice age by a high epeirogenic uplift of all of this continent northward from Mexico. The southern limit of this movement may perhaps be the east to west volcanic belt which includes Citlaltepetl (more commonly called the Peak of Orizaba), Popocatepetl, and other lower volcanoes.

3. In the interior of North America, the country comprising the great Laurentian lakes, the head of the Mississippi and Missouri rivers, and the plains stretching from the upper Missouri north to the Peace river, is known also to have been greatly uplifted and eroded during late Tertiary and early Quaternary time. Through the Tertiary era the area of these great plains had been subjected to wide and deep stream erosion, reducing the expanse from the upper Mississippi and the lakes of Manitoba west to the Rocky mountains by the removal of a thickness of the fresh-water Laramie beds, the highest member of the Cretaceous series, and the lower marine Cretaceous formations, to a depth of 500 feet or more about Turtle mountain in North Dakota and Manitoba, and not less than 3,000 to 5,000 feet, according to Davis and Wolff, about the Highwood and Crazy mountains in the northern part of Montana. Nearly all the region of the plains was thus reduced to a base-level of erosion, save comparatively small hill and mountain groups, by which we are enabled to measure the vertical extent of the general erosion. In the Pliocene period this expanse had probably become worn down nearly to sea level, but at the time of new epeirogenic uplift here considered it was raised, as Hilgard has shown for the upper Mississippi region, 3,000 feet or more above its present height. Rapid erosion by streams ensued with accompanying fluvial transportation and deposition; and to this time, contemporaneous with the Lafayette formation of the southern states, I refer the deposition of the Saskatchewan gravels described by McConnell in the basins of the Saskatchewan and Mackenzie rivers. With the increase and culmination of the uplift, immediately preceding and at last producing the Ice age, the wide flat trough of the Red river valley of the North, the low area of the Manitoba lakes, and the present valleys of the Assiniboine, Saskatchewan, Athabasca and Peace rivers, were channelled to depths 500 to 1,000 feet below the adjoining previously base-levelled plains. In the region of the Laurentian lakes the stream-cutting of the greatly uplifted country reached to similar depths, where the lake beds, once portions of the preglacial river valleys and basins, by crustal deformation during the Glacial period became bent down below the sea level.

4. Along all the northern shores of this continent, from Maine and Pu-

get sound north to the Arctic archipelago and Greenland, great preglacial altitude of the land is proved by the many fjords and coastwise channels; and between Cape Cod and Newfoundland the very irregular submarine contour of the Fishing Banks must be ascribed to subaërial erosion during the same time of continental elevation. The maximum vertical extent of this uplift, doubtless maintained during only a short time, is known to have been as much as 2,844 feet at the mouth of the Hudson river, as shown by the submarine continuation of that valley, and from 2,000 to 3,120 feet on the Californian coast, as shown by its similar submerged valleys. The duration of the high uplift, however, appears to have been far longer in the polar region than at these low latitudes. Corresponding with the narrow though deep Hudson and Californian submarine valleys, it seems probable that the Tertiary and early Quaternary stream erosion in the far north was sufficient to separate the Arctic islands from each other by the present wide channels, bays and sounds, which a subsequent depression filled with the sea.

The records of stream erosion in all portions of the United States and Canada thus testify of their epeirogenic uplift preceding and probably producing the Glacial period.

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THE EMERGENCE OF SPRINGS. By T. C. HOPKINS, Altoona, Pa.

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ADDITIONAL FACTS BEARING ON THE QUESTION OF THE UNITY OF THE GLACIAL PERIOD. By Prof. G. FREDERICK WRIGHT, Oberlin, Ohio.

[Printed in the American Journal of Science, 1893.]

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EXHIBITION OF TRILOBITES SHOWING ANTENNÆ AND LEGS. By CHARLES D. WALCOTT, U. S. Geological Survey, Washington, D. C.

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GEOLOGY OF HILLSDALE COUNTY, MICH. By HORATIO P. PARMALEE, Hillsdale, Mich.

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[Printed in the Natl. Geological Magazine, 1893.]

SECTION ACROSS THE COASTAL PLAIN REGION IN SOUTHERN NORTH CAROLINA. By J. A. HOLMES, Raleigh, N. C.

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SOME QUESTIONS RESPECTING GLACIAL PHENOMENA ABOUT MADISON. By T. C. CHAMBERLIN, Univ. of Chicago, Chicago, Ill.

SECTION F.

# ZOÖLOGY.

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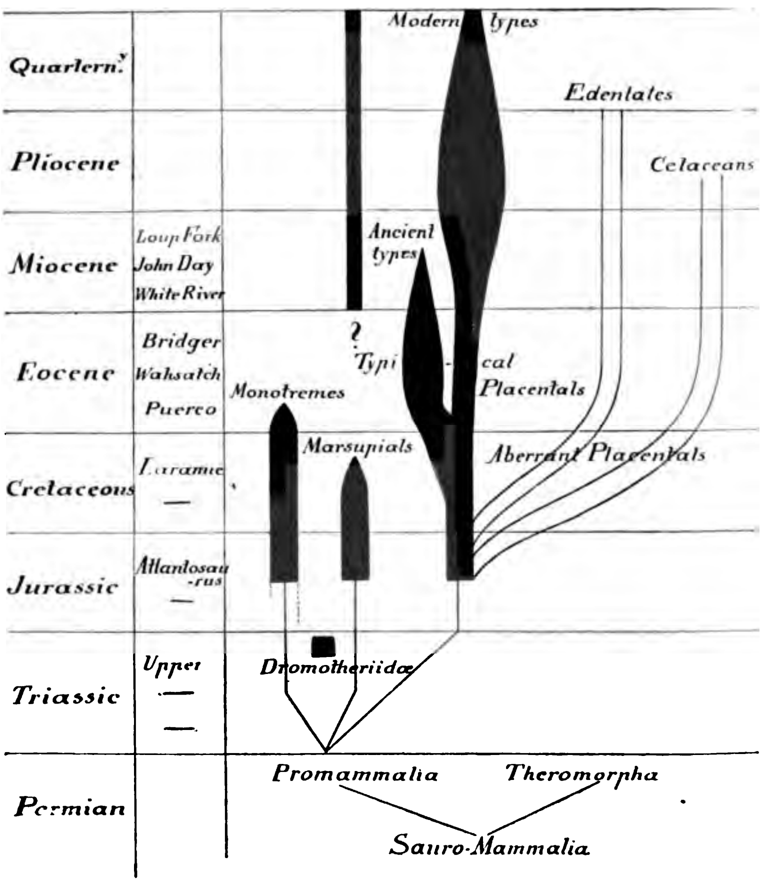



**HYPOTHETICAL PHYLOGENY OF THE NORTH AMERICAN MAMMALIA.**

**The Monotremes disappear in the basal Eocene. The Marsupials disappear in the upper Cretaceous and reappear in the lower Miocene (*Didelphys*). The Aberrant Placentals branch off in the upper Jurassic. The Typical Placentals divide into the "Ancient types" dying out in the Miocene, and the "Modern types" still existing. The Aberrant Placentals are given off in mid-Mesozoic times.**

# ADDRESS

BY

# HENRY FAIRFIELD OSBORN,

VICE PRESIDENT, SECTION F.

## *THE RISE OF THE MAMMALIA IN NORTH AMERICA.*

Twenty years ago an era opened in the mammalian paleontology of Europe and America. Partly inspired by the *Odontographie* of Rütimeyer, Kowalevsky completed and published in 1873 his four remarkable memoirs upon the hoofed mammals. He wrote these four hundred and fifty quarto pages in three languages not his own, in French upon *Anchitherium* and the ancestry of the horses, in English upon the *Hyopotamidæ*, in German upon *Gelocus*, *Anthracotherium* and *Entelodon*, including the first attempt at an arrangement of a great group of mammals upon the basis of the descent theory. These memoirs swept aside all the dry traditional fossil lore of Europe; they breathed the new spirit of Darwin, to whom the chief one was dedicated, making principles of descent of more importance than new genera and species. Kowalevsky thus summed up the contemporary paleontology:

"After the splendid osteological investigations of Cuvier had revealed to science a glimpse of a new mammalian world of wonderful richness, his successors have been bent rather upon multiplying the diversity of this extinct creation than on diligently studying the organization of the fossil forms that successively turned up under the zeal of amateurs and collectors. . . . . With the exception of England (referring to Owen, Huxley, Falconer, and others), where the study of fossil mammalia was founded on a sound basis, and some glorious exceptions on the continent (referring to Rütimeyer, Gaudry, Fraas, Milne-Edwards), we have

very few good paleontological memoirs in which the osteology of extinct mammals has been treated with sufficient detail and discrimination; and things have come to such a pass, that we know far better the osteology of South American, Australian, and Asiatic genera of fossil mammals than of those found in Europe."

At the same time, between 1871 and 1873, the pioneers of American paleontology, Leidy, Marsh, and Cope began the exploration of our ancient lake basins rich in life. The first ten years of their work not only revolutionized our ideas of mammalian descent, but brought together the data for the generalization of the second decade; for Marsh's demonstration of the laws of brain evolution in relation to survival; for Cope's proof of ungulate derivation from types with the simple foot resting upon the sole, and with the conic or bunodont ancestral molar tooth; and, finally, for Cope's demonstration of the trituberculer molar as the central type in all the mammalia. These four generalizations furnished a new working basis for morphology and phylogeny.

In these twenty years, thanks to energetic field work, we have accumulated vast materials for the history of the rise of the mammalia, enough for ten students where there is one, and the question arises, How shall we take best advantage of it, what methods shall we adopt? In this address, besides bringing before you the more recent achievements of exploration and research, I will try to illustrate the advances already made in lines of thought, observation and system in paleontology and indicate other advances which seem to me still desirable. In the problem of how to think and work most effectually and with most permanent results, all the sciences meet on common ground.

### ADVANCES IN METHOD.

It is to the renown of the veteran Rütimeyer and of Kowalevsky, so soon unfortunately deceased that, while their main inductions suffer by American discoveries, their methods of thought have not been displaced. It matters little that their theory, that ungulate molars sprang from lophodont or crested forms, has been disproved; that Kowalevsky's tables of descent are full of errors; that his main generalization as to the persistence of adaptive and extinction of inadaptive foot types does not hold good; that the horses and Anchitherium spring not from Palæotherium as he supposed, but from *Pachynolophus* and *Hyracotherium*, types which

he carefully studied and yet ommitted from the horse line. It is the right system of thought which is most essential to progress; better in the end wrong results such as the above, reached by the right method, than right results reached hap-hazard by a vicious method. If a student asks me how to study paleontology, I can do no better than direct him to the "*Versuch einer natürlichen Classification der fossilen Hufthiere*," out of date in its facts, thoroughly modern in its approach to ancient nature. This work is a model union of the detailed study of form and function with theory and the working hypothesis. It regards the fossil not as a petrified skeleton, but as moving and feeding; every joint and facet has a meaning, each cusp a certain significance. Rising to the philosophy of the matter, it brings the mechanical perfection and adaptiveness of different types into relation with environment, the change of herbage, the introduction of grasses. In this competition it speculates upon the causes of the rise, spread and extinction of each animal group. In other words the fossil quadrupeds are treated *biologically*—so far as possible in the obscurity of the past. From such models and from our own experience we learn to feel free to abandon traditions in the use of the tools of science, such as mere methods of description and classification, and to regard priority in nomenclature only.

New discoveries continually produce new conditions; there is nothing more obstructive than the reverence for old ideas and systems which have outlived their usefulness. In observation, an old principal was *de minimis non curat lex;* now, we cannot be too exact. Every cusp and facet has its value, not as a sign-post for a new species, but as suggestive of some function or relationship. Bird's-eye methods of comparison which, for example, find no difference between a rhinoceros and a lophiodon molar, are of no service now that we are called upon to distinguish between so many lines of ancient mammals crowding in among the ancestors of existing mammals. Again, paleontology is not a science apart; it has always gone hand in hand with recent osteology; it must now keep abreast with the embryology of the teeth and skeleton; with the animal mechanics of Marey, Allen, and Muybridge; with paleobotany, geology, and historical-physical geography. In these points we cannot be too broad. All structures should be considered as to their homologies, their mechanics, which throw such a brilliant light upon their evolution, their relations to the

food and soil and to other parts. This brings us to the animal as a whole—its tendencies, its place in the system of descent, its relations to its contemporaries, the causes of its progression or retrogression; finally into pure speculation. Here I am reminded of a critical saying by the late Professor v. Gudden, the distinguished neurologist: "Ein Steinchen der Wahrheit hat mehr Werth als ein grosser Schwindelbau;" it was an allusion to the temporary character of the great nerve-tract systems of Meynert and Flechsig. The great "Schwindelbau," literally the "disappearing structure" of paleontology, is the phyletic tree which adorns the end of many good as well as superficial papers; and recently, because of its extremely brief life, has fallen somewhat into disfavor. I do not think the present reaction against these "trees" is a wise one; we must remember they are the working hypotheses of our branch of science and serve to express most clearly present knowledge.

To illustrate some of these principals of modern methods, let us first look at the evolution of the teeth in the rise of the mammalia. The teeth and the feet are the foci of mammalian evolution, the only direct points of contact with food and the earth. Their combined use in phylogeny has increased in interest, because their evolution has proved wholly independent. We recall Cuvier's famous law, of which Balzac said at the time: "Rebuilt like Cadmus cities, from a tooth."

No generalization has been more thoroughly routed than that of a necessary law of correlation between tooth and foot structure. Beside the orthodox clawed carnivores and hoofed pachyderms of the great French anatomist, we have discovered hoofed carnivores such as Mesonyx, and clawed pachyderms such as Chalicotherium. Even the apparently lasting barriers of correlation, which Owen raised between the even and odd-toed ungulates, have broken down by Ameghino's discovery of a Litoptern odd-toed horse with an even-toed type of astragalus. Not only is there no correlation of type, but none in the rate of evolution. Hipparion, the most progressive horse in tooth structure, probably owed its extinction to the conservative preservation of its ancestral three toes. For these reasons the teeth and feet, owing to the frequent parallels of adaptation, may wholly mislead us if taken alone; while, if considered together, they give us a sure key; for no case of exact parallelism in both teeth and feet between two unrelated types has

yet been found, or is likely to be. This, I believe, is the one lesson of later work which reverts to older methods; we should not base either classification or descent upon the teeth or feet alone. Every additional character diminishes the chances of error.

The evolution of foot structure has now become a science and advances have been made in the principles of progression from the plantigrade, pentadactyl serial types to the unguligrade, monodactyl alternating types which are of the greatest importance in classification and phylogeny. It is surprising how little attention was given to ungulate foot structure between the time of Cuvier and Kowalevsky. Owen's generalization as to the Artiodactyl and Perissodactyl pes formed the one bright exception. Kowalevsky first directed attention to the importance of the more median metacarpals displacing or spreading to gain a stronger foothold upon the carpals as the lateral toes disappeared. Ryder also worked out the laws of reduction. The discovery of Phenacodus led Cope to the final generalization that the primitive ungulates were not only plantigrade but had some of their carpals and tarsals in vertical rows like bricks clumsily set with unstuck joints—and that one great law of evolution towards digitigradism was to produce diplarthry or alternating joints. As he found this alternation differed both in degree and kind in different groups, he revived the comprehensive "Ungulata" of Linnæus and divided all hoofed mammals exclusively upon their foot structure into five great orders.

Rütimeyer and myself have shown that however successful and convenient this system appears, Cope's lines of division ignore the fundamental different modes of evolution of the fore and hind feet; an animal may be taxeopod in front and a diplarth behind or *vice versa*. Numerous exceptions to Cope's definitions are also found. The discovery of the aberrant ungulate foot types of South America further invalidates Cope's system and sustains the principal that to be permanent classification must be based upon at least two entirely diverse sets of characters. This does not diminish the importance of the primitive taxeopod plantigrade type as one great key to the still unsolved problems of the primary relationships of the Condylarthra, Hyracoidea, Amblypoda, Proboscidia, Toxodontia, Litopterna, Artiodactyla and Perissodactyla. All these orders still stand apart in the dim past like so many mileposts.

While Cope overestimates the feet in these larger divisions,

many writers in Europe still depend wholly upon the teeth and ignore the wide degrees of divergence such as are indicated in the Perissodactyla, for example, in functional tetra-, tri- and monodactylism. By "functional" we refer to *tendencies* which are not expressed in the bare digital formulas—and which have the same relation to the feet that the dental curve has to the teeth. The evolution of a monodactyl tendency is not the work of a century but of a geological period, a principal which we wholly ignore when we place the monodactyl Anchitheres with the tridactyl Palæotheres, on the ground that their dental type and digital formulæ are identical. How many toes an animal has is of far less importance than how these toes are being displaced and reduced.

### LOWER MESOZOIC PRO-MAMMALIA.

With the exception of the Triassic *Theriodesmus* of Seelye, no mammal is known by its limbs or skeleton until we reach the basal Eocene; in studying the first steps in the rise of the mammalia, we are thus practically driven to the teeth and jaws alone. In these straits of the fossil-hunter, embryology has lately come famously to aid.

Assuming their remote reptilian origin, agreeing with Baur and Kükenthal that the theromorph reptiles were parallel with, rather than ancestral to, the mammals, and therefore placing before both groups the hypothetical *Sauro-mammals* in or below the Permian, we come to, the old question which Huxley discussed in his famous anniversary address: "Was there a succession between Monotremes, Marsupials, and Placentals, or a parallel development from a common promammalian type?" Then we look to the newer questions, "When were the Edentates and Cetaceans given off?"

Modern tooth-science springs first from the recent demonstration of Rütimeyer's hypothesis of 1869, that the teeth of all the mammals centre around a single reptile-derived type. With a single exception, which I believe can be disposed of, various stages of trituberculism or a three-cusped condition have become the standard for the teeth, as pentadactyly has long been for the feet, except that this is developed within the mammalian stem, while our five fingers are a reptilian legacy. Second, it springs from the recent thorough exploration of the youngest jaws for evidences as to the primitive form and succession of the teeth.

This also supports the reptile theory of tooth descent by proving, what has been in considerable doubt, that the Promammalia had a multiple succession of teeth like the reptiles, and that even some of the modern mammals retain dim traces of four series of teeth.

The brilliant discoveries of Kükenthal, Leche, and Röse begin to show how in various ways the mammals early modified the regular succession of all the teeth by suppression of parts of the multiple series; this is the first thing to consider. The next is how heterodontism arose, how the conic rows of teeth were specialized in different parts of the jaw for three or four functions; as a certain number of teeth took up each function, the question arises whether this number of dental formula was ever the same in all the mammals, for we know it is very different now. After the teeth were thus divided, some functions became more important than others, and established a monopoly, causing first a marked difference in the relative development of the series, which we may express in a dental curve, resulting finally in the loss of certain teeth. In the meantime began the special evolution of the form of the back teeth, or molars. Was this alike in all mammals, was it tritubercular? It is surprising how many problems of early relationship are at stake in these simple processes.

### *Primitive Diphyodontism.*

What does *succession* really consist in? It now appears that Baume was right in denying that the first tooth is mother of the second; for the teeth of the lower as well as the upper series, spring from the common epithelial dental fold (Schmelzleiste) which drips down from the surface and extends the whole length of the jaw; at intervals it buds off the dental caps (Schmelzkeim) of the first series; after these are separated off, the dental fold sinks and buds off the dental caps of the second series, always below and inside the first; thus the fold is the mother and the caps are sisters, twins or triplets, according to the number of the series. In all young mammals, including the traditional monophyodont Cetaceans and Edentates, and excepting only the still unexplored Monotreme embryos, traces of the two series of teeth have been found. Both Leche and Röse have detected evidence that the dental fold sometimes buds off parts of a third series, thus explaining the occasional reversion of supernumerary teeth on the

inner side of the second series, and Leche has seen traces of budding preceding the first series—thus giving us vestiges of four successions.

All our perplexities as to the relation of the milk and permanent teeth, and the ingenious but mistaken hypotheses of Baume, Flower, Wortman, and Cope have sprung from our want of evidence of the regular and complete diphyodontism of the stem mammals. The solution in brief is that the "milk teeth" and the "true molar" are descended from the first series, while the second series is represented by the " permanent incisors, canines, premolars" and rudiments of dental caps beneath the true molars. The mammals early began to diverge from the primitive diphyodontism in many ways; apparently adapting the first and second series, respectively to their infant and mature feeding habits; loosing parts or all of one series or the other, and in some cases pushing teeth of the second series in among the first; this intercalation has been a most confusing factor to us.

In the Marsupials (Kükenthal) almost the entire first series became permanent; thus from the Jurassic period to the present time only a solitary fourth premolar of the second series has pushed out its elder-sister tooth, and Röse has observed that an outer upper-incisor also pushes up from the second series; the remainder of the second series still persist as rudimental dental caps beneath the first, even beneath the first and second molars. There are wide variations among the Placentals; thus in the lowest existing forms, the Insectivora, Leche finds that in the shrew (*Sorex*) the second series is suppressed entirely, while in the hedgehog (*Erinaceus*) of the twelve permanent teeth in the anterior part of the jaws five belong to the first series and seven to the second. We thus meet with the paradox, that among the "primitive" Marsupials and Insectivores the regular reptilian succession was early interrupted, while in all the "higher" mammals the reptilian succession of two series was retained in the anterior part of the jaw. Beneath the posterior highly-specialized molar teeth of both Marsupials and Placentals, the second teeth were early suppressed, although in the Edentates, which also originally had specialized molars, there is a typical succession of seven teeth behind the canine. These discoveries prove that the whale teeth, like their paddles, have acquired a secondary adaptive resemblance to those of the Icthyosaurs. How did the single and simple teeth

of the Edentates and Cetaceans develop? Clearly by retrogression. As Leche points out in the aquatic Carnivora, in which the first series are degenerating, the single-series condition (monophyodontism) advances step by step with retrogressive simplification of the tooth form (homodontism); thus in the true seals, the eared seals and the walruses, as the permanent teeth became simpler, the milk teeth becames maller. The Edentates, so widely separated gentically, parallel the seals in tending to suppress the first series of teeth and simplify the crowns of the second series at the same time. We might jump to the conclusion that this gives us an explanation of the homodont and apparently monophyodont condition of the toothed whales, especially as it has been supposed they sprang from aquatic Carnivora, but in this order matters were reversed, for the first series persisted and the second series were suppressed and persist as a rudimental row of tooth caps buried in the jaw.

Each dental series has an adaptive evolution of its own, in Erinaceus the first series has an ancient and the second a modern form; in Ericulus both series are alike; in the bats the first series is homodont; the second is heterodont (Leche); in the Edentates the first series is ancient and heterodont; the second is modern and homodont (Thomas, Rheinhardt); so among the Cetaceans and Ungulates.

What deep and ancient clefts the different laws of succession mark between the Marsupials and these three Placental groups.

### *Primitive Heterodontism and Formula.*

Now that all mammals are led back to a distant diphyodont stem, it is also true that the further we go back both in palingenesis and embryogenesis, the more widespread heterodontism is —all modern homodontism proving to be secondary. The simple conic teeth of the porpoise, for example, bear a misleading resemblance to those of a reptile. Flower, Weber, Julin and Kükenthal agree that the ancestral whales and edentates were heterodont and had a smaller number of teeth than the existing forms.

Heterodontism is then the second problem. When did the division of the teeth into incisors, premolars, and molars occur, before or after the Monotremes, Marsupials, and Placentals separated? It is well settled that the canine was the first maxillary

tooth and developed from the most anterior bi-fanged premolar; also, from the discovery of complete succession, we must now define the first molar as the most anterior specialized or triconid tooth, not as the most anterior permanent tooth. It seems to me we now find strong evidence that the stem mammals had a uniform number of each kind of teeth; in other words, a uniform dental formula. The Monotremes are most in doubt as the existing forms point only to primitive heterodontism. It will be a great step forward when we learn whether or not the multituberculates are Monotremes—the resemblance of their molars to those of the duckbill is very superficial, for the duckbill's upper molars lack the intermediate row of tubercles universally seen in the multituberculates, and look to me rather like degenerate trituberculate teeth. Cope has recently found in the cretaceous rocks a remarkable trituberculate, which he names *Thlæodon;* the jaw of this animal is neither Placental nor Marsupial; it is like that of the multituberculates—and both resemble remotely the degenerate modern monotreme jaw. All we can say, therefore, is that the multituberculates are an archaic group, highly specialized even in the Trias, that they were *probably* Monotremes, and neither structurally nor functionally akin to the diprotodont marsupials (Owen) nor the Microbiotheridæ (Ameghino). With a dental mechanism and a condyle exactly like that of the rodents, they show no trace of canines, and the mode of evolution of their peculiar molars was probably paralleled later in the rodents. They present vestiges of a primitive dental formula, like this: *I*3,*C*? *P*4, *M*4,+. *Thlæodon* shows *C*1, *P*4, *M*3. Thus, so far as this doubtful paleontological evidence goes, the Monotremes had a typical formula.

Our next step is to unify the typical 5. 1. 3. 4 of recent Marsupials with the 3. 1. 4. 3 of higher Placentals. Thomas has shown in his studies of recent Marsupials that they have probably lost one of the four typical premolars (*pm.* 2); this observation, fortunately, is partly confirmed by Röse's finding an embryonic germ of this tooth. Ignoring the incisors of the Jurassic Marsupials, Thomas raised the number of ancestral incisors to five, the highest number known among recent Marsupials; Röse therefore made another step toward uniformity when he showed that the Marsupial *i*5 is probably a member of the second series of incisors, and should not be reckoned with the first. Now, if we suppose

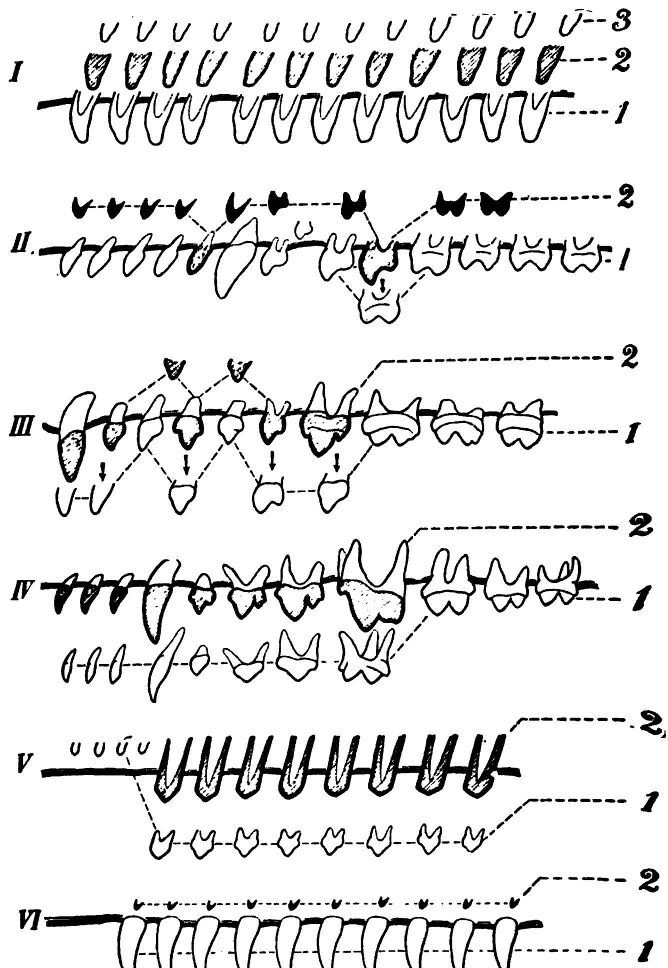
that the Placentals have lost one incisor and one molar, abundant evidence of which is found in Otocyon, Centetes and Homo, we derive as the ancestral formula of both orders:

Incisors, 4; canines and premolars, 5; molars, 4.

The aberrant placental Cetacea point in the same direction as we read in the conclusion of Weber's fine memoir: "All the Cetacea sprang from a stem with a heterodont, but only partly specialized dentition (something like that of Zeuglodon, 3. 1. p. and m: 7), . . . not direct from Carnivores or Ungulates, but from a generalized mammalian type of the Mesozoic period, with some affinities with the Carnivora. . . . Zeuglodon itself branched off extremely early from the primitive line, and the heterodont Squalodon (mark its formula, 3. 1. 4. 7.) "branched off later from the toothed whale line, after the teeth had begun to increase in number and before homodontism had set in." It would be easier for us while speculating to take Squalodon and the Odontocetes directly from the Jurassic mammalian formula (3. 1. 4. 8.). As for the multiplication of this formula, we have found the way, says Kükenthal, by which numerous homodont teeth have arisen from a few heterodont molars, *it is by the splitting up of the numerous triconid molars of Jurassic ancestors into three.* He substitutes this hypothesis for the one advocated by Baume, Julin, Weber, and Winge, that the multiple cetacean teeth represent the intercalation or joint appearance of both the first and second series of teeth, owing to the elongation of the jaw—a view which is now disproved by Kükenthal's discovery of the second row beneath the first. Since even by Kükenthal's hypothesis the typical Mesozoic mammal could not furnish as many teeth as are found in some of the dolphins, a likelier explanation than his seems to be that as the jaws were elongated the dental fold was carried back and the dental caps were multiplied.

The Edentates, like the Cetaceans, point back to heterodontism, and somewhat less clearly to a typical dental formula. We are here indebted to Flower, Rheinhardt, Thomas, Kükenthal, and Röse. It is their rudimental and useless first series which gives the evidence of heterodontism, while the second series has become adaptively rootless and homodont. The especially aberrant feature is that a double succession exists in the typical "true molar" region. The adult nine-banded Armadillo presents only eight maxillary teeth, seven of which are preceded by two-rooted milk teeth (Tomes); in the embryo Leche finds fifteen dental caps, of

which only thirteen are calcified; this number probably includes the four rudimentary incisors observed by Rheinhardt. In the




RELATIONS OF THE FIRST AND SECOND SERIES OF TEETH.

*I.* Reptiles. *II.* Marsupials. *III.* Insectivores (*Erinaceus*). *IV.* Higher Placentals. *V.* Edentates. *VI.* Cetacea. Odontocetes.

aberrant Orycteropus (Aard-Vark), with ten adult teeth, Thomas finds seven milk teeth behind the maxillary suture (thus taking us into the molar region of the typical heterodonts). The last of these milk teeth is large and two-rooted; behind this are three

large permanent posterior teeth, apparently belonging to the first series. The large lateral tooth of Bradypus is suggestive of a canine. From this rapidly accumulating evidence it appears probable that the ancestral Edendates had four incisors, a canine and eight or more teeth behind it, the double succession extending well back so that the first series did not become permanent at the fifth tooth behind the canine, as in the Marsupials and higher Placentals. If these are primitive conditions, as seem probable from comparison with fossil Edentates, they carry the divergence of the Edentates, like that of the Cetaceans, back into the Mesozoic period. Comparative anatomy and embryology thus point back to highly varied branches of a generalized placental heterodont stem in the Mesozoic, and a much earlier divergence than we formerly imagined. Now let us see what the early Mesozoic mammals point forward to.

There are three distinct and contemporary Jurassic types, the multituberculates, the triconodonts, and the trituberculates. Are not these the representatives of the Prototheria, Metatheria, and Eutheria? In the archaic multituberculates we have seen a monotreme type of jaw and vestiges of a typical ancestral formula. The tricondodonts are a newer group, perhaps derived from the Dromotheriidæ (incipient triconodonts) of the Trias although these appear to be aberrant; the typical forms extend from Amphilestes to Triconodon, and exhibit the first stages of development of the inflected Marsupial jaw. The trituberculates include the Amphitheriidæ and Amblotheriidæ with true tuberculo-sectorial lower molars, like those of modern Insectivores; they alone exhibit the typical angular placental jaw,—no reason can be assigned for calling them Marsupials, excepting the traditional reverence for the Marsupial stem theory. Now, it is very significant that the average dentition of these old but highly diverse forms, namely, Multituberculates, 3. ? 4. 6., Triconodonts, 4. 1. 4. 7., Trituberculates, 4. 1. 4–5. 8., is also the dentition to which the existing mammals apparently revert.

The third problem is from what type of molar tooth did the mammalia diverge?

## *Primitive Trituberculism.*

There is a very general tendency among the vertebrates as a whole, fishes and reptiles as well as mammals, to form what are

called "triconodont" crowns by the addition of lateral cusps to simple cones. In the mammals alone, these three cusps pass into higher stages of evolution, through what is called "tritubercnly" in which these cusps form a triangle. The discovery of primitive wide-spread tritubercnly, by Cope, was a great step forward. In looking over the odontographies of Cuvier, Owen, Tomes and Baume, we find there is no suspicion of this common type around which the highly diverse mammalian molars centre. The molars of the clawed and hoofed mammals can now be compared, as we compare the hand or foot of the horse with that of the cat, because they spring from a common type. All the specialized mammalian series, ungulates, primates, carnivores, insectivores, rodents, marsupials, are found playing similar yet independent adaptive variations upon one type. We thus have a key to the comparison of all molars with each other and with the reptile cones; take the human grinders, for example: the anterior outer cusps in the upper jaw and the anterior inner cusps in the lower jaw are homologous with each other and with the reptilian cone. Leaving aside for the moment the Multituberculates and Monotremes, every known Triassic, Jurassic, Cretaceous and basal Eocene fossil (excepting Dicrocynodon) is in some stage of tritubercnly; all the known Cretaceous molars are simple triangles above; all later fossil mammals also converge to tritubercnly, until in the lowest Eocene, every molar is tritubercular, and the early stages of divergenee are so similar that it requires a practiced eye to distinguish the molar of a monkey from a horse. Embryology supports the evidence of these fossil series; thanks to the recent admirable researches of Röse and Taeker, we find in the primates, ungulates and marsupials, that every molar in the calcification of its dental caps is heralded by *three cones placed in a triangle*, and in the lower jaw these three cones invariably appear in the same order (protocone, paracone and metacone) in which they arose during the remote geological periods.

It is necessary to mention this overwhelming paleontological evidence, because "tritubercnly" is still not universally recognized; Fleischmann and others have questioned the homologies of the upper and lower triangles, and two able writers, Röse and Forsyth Major, have independently proposed an opposition theory that "multitubercnly" and "polybuny" is the mammalian archetype, the latter author believing tritubercnly has become a "dogma." So far,

however, from there being any decline of evidence I am now able to add the Cretaceous mammalia to the tritubercular lists and bring forward evidence that the multitubercular molar instead of being primitive was derived from the tritubercular; moreover, all the researches I have been quoting tend to draw the mammals without exception into one of three great primary forms. The haplodont form, from which Dromotherium is just emerging in the Trias, is the oldest and nearest the reptiles; the triconodont or three cones in line, was a predominating lower Jurassic type; the tritubercular or three cones in a triangle (trigonodont Rütimeyer), was the prevailing upper Jurassic and later form. The final predominance of the tritubercular over the others was due to its possibilities of mechanical adaptation to work of every kind —its *potential* in evolution. Upon the polyphyletic theory of the origin of the mammals here advocated, we must admit, first, the independent evolution of trituberculy in different phyla and second, the branching off of several great groups in the pre-tritubercular stages.

In the problem of precedence of type, I admit that as to antiquity there is nothing to choose. The contemporaneity of the Rhætic Microlestes (a plagianlacid multituberculate) and the upper Triassic Dromotherium (an early trituberculate) is a puzzling circumstance; for, by my hypothesis, Microlestes was even at that time specializing from a more primitive trituberculate ancestor. The molar of this little animal is a narrow paucitubercular basin, not unlike that of several existing Rodents which are of undoubted tritubercular origin; and it is among the Rodents we find the explanation of the multituberculate molar. The molar of the mouse (*Mus*), and of certain kangaroo rats (*Dipodomy* and *Perognathus*), illustrate beautifully the recent stages between trituberculy and multituberculy, showing that the intermediate tubercles of *Mus* (also common in other placentals) give rise to the intermediate or third multituberculate row.[1] Then each row is fortified by additional tubercles; so that, finally, Perognathus, with its longitudinal rows of cusps and grooves, is in a similiar stage of evolution to Tritylodon of the upper Trias of South Africa. This proves that *the tritubercular molar has the potential of a typical multitubercular*. Add to this the fact that the premolars of many multituberculates (Ctenacodon, Bolodon, Chirox) are tritubercular, and we have strong indirect evidence that the multituberculates had

[1] Prof. J. A. Allen and Dr. J. L. Wortman kindly assisted me in this comparison.

trituberculate ancestors. As for Röse's fusion theory of multituberculate origin, it may be pointed out that the oldest types, with an abundance of primitive reptilian cones to fuse, have only five or six cusps, while the newest types, remote from the reptiles, have as many as twenty-five cusps. Fleischmann's objection is of a different character; he believes, from his studies of the Insectivora, that Cope and myself have mistaken the homologies of the parts in the upper and lower molars, and endeavors to show that the posterior end (talon) of the lower molar is equivalent to the anterior end (trigon) of the upper molar. His position is shown untenable by a study of Spalacotherium and other Jurassic types in which there is no talon below or above, and it is proved that the upper and lower trigons must be homologous. The teeth of this form should also settle Röse's doubts as to the position of the reptilian protocone in the upper and lower jaws.

Examine evidence of another kind as to the primitive type. Retrogression inverts the order of evolution. We know this of *Thylacinus*, in which a tritubercular molar turns back into a triconodont. In the aquatic Carnivora, the seals, eared seals, and walruses, the triconodont is also retrogressing into the haplodont. The inference is a fair one that the aquatic like the terrestrial Carnivora, were originally tritubercular. With the Cetacea, both paleontology and embryology take us back to a more or less typical triconodont molar, not to the tritubercular. The Edentates also give feebler evidence of ancestral triconodont or tritubercular molar forms.

Thus, the tendency of recent research is to show that all stem mammals were related in their double succession, in their dental formula, and in their primitive molar form. These features point, not to a succession, but to a unity of ancestry of the Monotremes, Marsupials, and Placentals.

### *Divergence of the Three Groups.*

The discovery of the complete double series seems to have removed the last straw from the theory of the Marsupial ancestry of the Placentals, for the peculiar mode of suppression of the second series in the Marsupials has been constant since the Purbeck; this difficulty is added to the structure of the jaw, the epipubic bones, the profoundly different mode of fœtal nutrition. None the less, any conclusion we can draw now as to the primary relations of the three great groups is more or less of a "Schwindel-

bau," and I put together the results of these later discoveries with a full realization of the temporary character of present conclusions.

The Permian Sauro-mammalia (Baur) with a multiple succession of simple conical teeth divided into : *A*. Theromopha, which lost the succession and in some lines acquired a heterodont dentition and triconid single-fanged molars. *B*. Promammalia.

The hypothetical Lower Triassic Promammalia retained a double succession of the teeth; they become heterodont, with incipient triconid double-fanged molars; dental formula approximating 4. 1. 4–5. 8. They gave rise to three groups: I. The Prototheria which passed rapidly through the tritubercular into the multitubercular molars into the line of multituberculates, and more slowly into trituberculy and its later stages in the line of Monotremes. II. The Metatheria or Marsupials tended to suppress the second series of teeth, except those intercalated with the first; by this and by reduction the formula become 5. 1. 3. 4–6; the molars passed slowly through the triconodont into the typical tritubercular type. III. The Enthera or Placentals divided early into a number of branches, in which there was heterodontism, but no uniform modification of succession namely: *A*. Forms suppressing the second series in the molar region only and acquiring a typical Entherian dentition, 3. 1. 4. 3–4. 1. The Insectivores tended to partly suppress the anterior teeth of the second series or intercalate them with teeth of the first series; the molar becomes tritubercular. 2. The higher Placentals retained the succession of the first and second series as far back as the first molar; the molars entered rapidly into trituberculy and its higher stages. *B*. Forms retaining the double succession in part of the molar region, and retaining more of the primitive dentition, 4. 1. 4. 8. 3. The Edendates branched off from an early triconodont or tritubercular diphyodont stage, with numerous molars, and secondarily suppressed the first heterodont series and established a numerous homodont second series. 4. The Cetacea also branched off from a diphyodont, heterodont stage, and secondarily established a numerous homodont first series, and suppressed the second series.

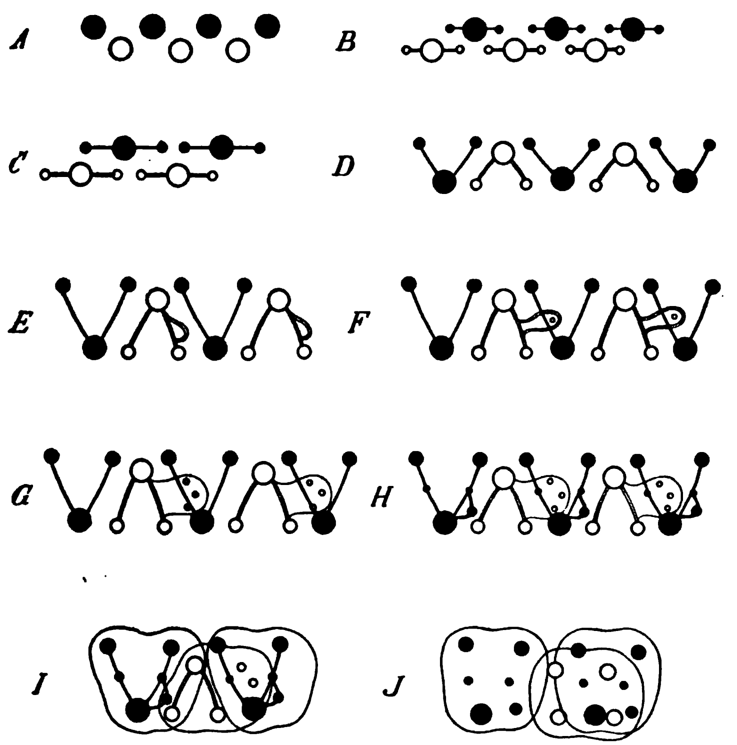
## ORIGIN AND EVOLUTION OF TRITUBERCULISM.

"Concrescence" is the newest theory of cusp evolution—an expansion by Kükenthal and Röse of views earlier expressed by Gau-

dry, Magitot and Dybowski. As Kükenthal derives three conical Cetacean teeth by splitting apart a triconodont molar, he conversedly derives a triconodont molar by bringing together of three reptile cones. Smith Woodward has called attention to the support the epidermal structures of the fishes give to this hypothesis, yet as applied to mammalian teeth, it comes from a one-sided morphology which regards only the wonderful though mutilated chapters of embryology when the untorn pages of paleontology are at hand. Between the Trias and the Puerco, we are, so to speak, in at the birth of every successive cusp and can observe positively that the law of cusp evolution is direct upgrowth from the smooth slopes of the crown or from the cingulum, that fertile parent of new cusps. Each new cusp is usually preceded by an abraded surface and prophesied by an excessive minute hillock. It follows from this that cusps range in size and height directly according to their age—a principle beautifully demonstrated in some of the Mesozoic teeth. If the Kükenthal-Röse theory were correct, the oldest triconodont should be iso-conid, whereas we know that the three equal cones of Triconodon are all a very late development; the earlier forms show the lateral cones receding to the needle-points of Dromotherium.

The tritubercular molar owes its survival to the original advantage of its triangular form, and to the possibilities of free cusp addition—as worked out by Cope, Wortman, Schlosser, Scott and myself. Rütimeyer's term, "trigonodont," best expresses the primitive structure of the upper and lower teeth, as of two interlocking triangles with their open bases turned outward in the upper and inward in the lower jaw. These "trigons," cutting past each other, made a shear so perfect that many Insectivora retained it without further evolution. But in most trituberculates a talon was next added to the lower molar (Jurassic stage) as a pestle crushing into the upper valley; this talon gradually widened into a broad heel supporting three cusps, as found in the Cretaceous. Consider the extreme antiquity of the three homologous cusps borne upon the back part of the human molar. This addition gave the opposed molars two shears and one crusher, and was so perfectly adapted to the needs of Lemurs and many Insectivores and Carnivores, in short of most clawed animals, that they stopped at this point. Not so with the Herbivora, which required more extensive crushing surfaces. The upper molars, which had remained

triangular through the Cretaceous and into the basal Eocene, began to develop a little talon, like that early seen in the lower molars, and at the same time both upper and lower molars entirely sacrificed their primitive cutting powers, and were converted from secodont into bunodont types by bringing the primitive



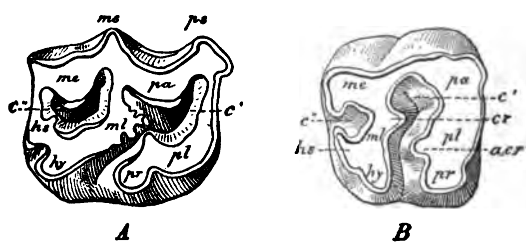

PHYLETIC HISTORY OF THE CUSPS OF THE UNGULATE MOLARS.

*A.* Reptilian stage, Haplodont, Permian. *B.* Protodont stage (*Dromotherium*), Triassic. *C.* Triconodont stage (*Amphilestes*). *D.* Tritubercular stage (*Spalacotherium*). *E.* Tritubercular-tuberculo sectorial, Lower Jurassic. *F.* The same, in Upper Jurassic. *G.* The same, in Upper Cretaceous. *H.* The same, Puerco, Lower Eocene. *I.* Sexitubercular-sexitubercular, Puerco. *J.* Sexitubercular-quadritubercular, Wahsatch.

trigons down to the level of the talons. At the same time, the upper molars acquired intermediate tubercles, and the triangular or oblique arrangement of the tubercles was shifted into the quadrangular or transverse arrangement. This outline is the result of fifteen year's observation.

With square crowns (*vs.* triangular) and six conic cusps above

and below, the molars of the Artiodactyl and Perissodactyl Herbivora ended their first constructive stage at that period, and started upon their modernization. From this point we direct our attention upon the numerous combinations of three or four forms assumed by these single cones. The important thing now is to determine at what period these combinations were established, for there is wide difference of opinion as to when ungulate divergence began. To this I refer later. Taeker has recently shown how every modern embryonic lophodont or selenodont molar first exhibits the archetypal cones of the primitive bunodont. This law, together with my own parallel studies of the evolution of the horse




THE LIMITS OF VARIATION.

*A. Merychippus. B. Aceratherium.* Showing the secondary enamel foldings of the crests arising from the centres of the ancestral cones.

and rhinoceros molars, led me to the discovery that these *embryonic primitive cones are also the main growth centres* for, in the Upper Miocene, long after the Perissodactyla have separated from each other, we see the influence of the archetypal form in the generic and specific variations of the molars. Compare the teeth of *Merychippus* and of *Aceratherium*, and imagine that you see underlying the diverse crests and crescents the simple bunodont molar of such a form as *Hyracotherium leporinum* of the London clay. You will then notice that the characteristic secondary folds and spurs of the Miocene teeth spring from the old bunodont cones, that the two "cement lakes" of Merychippus are equivalent to the two "fossettes" of Aceratherium, because the "crescentic spurs" of the horse and the "crochet" and "antecrochet" of the rhinoceros spring alike from the primitive "intermediate tubercles."

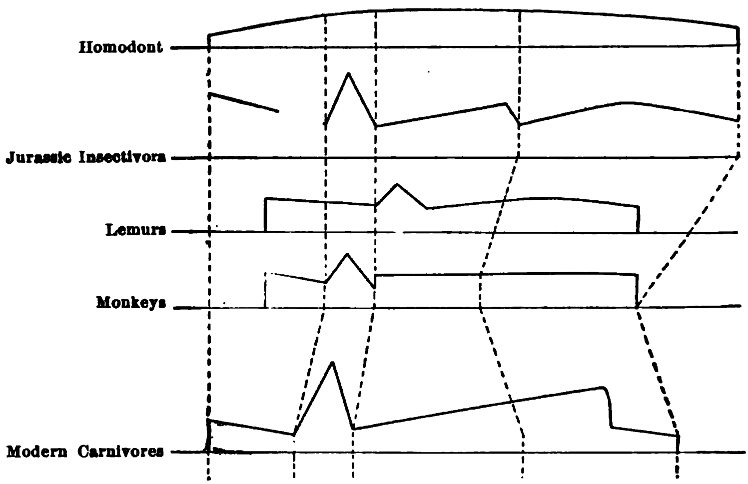
In view of these discoveries of the uniformity of mammalian molar type, a uniform terminology has become as necessary for the dental cusps as for the carpal and tarsal elements of the feet.

Professor Gaudry's once admirable system, elaborated in his "*Enchaînements*," was based upon the supposed division of the ungulate molar into the "first lobe" and "second lobe," and is still followed in France. Yet it has two drawbacks: it precludes the comparison of the ungulate with the unguiculate molar, for neither lobe includes the complete triangle; still more inconvenient is the fact that we cannot compare the higher Ungulates with the older Coryphodons and Periptychidæ in which the molars were developed upon the triangular plan; these teeth have only the first lobe and half the second. The upper molars of Hipparion and Coryphodon illustrate the advantages of this new system of comparison and of terminology.

Scott has made a further advance in odontology by working out the laws of premolar evolution or cusp addition. In many groups we know that from one to four of the premolars gradually acquire the exact form of the molars in order to further increase the grinding surface, and we should *a priori* expect that the cusps would be added in the same order, and therefore be homologous with the molars. This, as Schlosser and myself had observed, is not the case. Scott shows the order of cusp development in the premolar is very nearly the same in all the mammals, and yet is entirely different from the order followed in the molars. This law again unexpectedly ties the clawed and hoofed mammals together; the sequence of cusps in palingenesis is so similar to that observed by Taeker in embryogenesis, and Scott is justified in proposing a new terminology (protocone, deuterocone, trittocone, etc.) for the premolar cusps, which will in the end prove to be a great convenience.

I alluded above to the well-known extreme and very confusing similiarity of the tritubercular molars in the early stages of divergence. Trituberculism is at once the cause of clearness and of doubt when we get back to the stem mammals of widely different phyla. This has led to strange misconceptions of phyletic affinities as exemplified in Filhol's division "*Pachylémuriens*," a supposed mixture of lemur and ungulate stock. There was never any such mixture, and the question comes up how to distinguish unlike forms with like teeth? I have proposed to make use of a *dental curve* which will express the incipient atrophy of some parts, and hypertrophy of other parts of the series, a metatrophrism which will naturally terminate in the reduction of some teeth, and

excessive development of others. This has not been by any means fully worked out, but I believe it will prove to be of great service in directing attention to some of the initial tendencies of divergence, which are not expressed either in the dental formula or in the patterns of the teeth. Below are some of these curves.




EXAMPLES OF DENTAL CURVES.

When worked out by the composite method, we will find certain primary curves characteristic of the ordinal divisions, and minor curves distinguishing the lesser divisions. Of course the laws of parallelism will also be found in force here; flesh-eating, insect-eating, and grass-eating animals will be apt to have similar curves even when evolved in different groups, but here the dental formula and succession will come to our aid.

## BREAKS AND LINKS IN THE MESOZOIC FAUNA.

By our hypothesis all three sub-classes flourished together during the American Mesozoic; the Marsupials disappeared, then the Monotremes, and by the end of the basal Eocene the Placentals were in exclusive possession of the nothern continent.

Although we have great reason to congratulate ourselves upon the rapid progress of discovery, there still remain great gaps in Mesozoic time between certain horizons and in the lineal phyletic series of both the Mesozoic and Cenozoic. For a time stan-

dard we may take advantage of the remarkab'y constant evolution of the Plagiaulacidæ in the Mesozoic, and of the Equidæ in the Cenozoic—as certain invertebrates are made use of in older rocks. The grooves and tubercles of Plagiaulax and the cusps and styles of the horses are added with the precision of clock work, and supposing that the rate of evolution has been about the same, we can approximately estimate both the periods of deposition and the intervals as below.

PLAGIAULACIDÆ.

| | Stonesfield. | Purbeck. | Laramie. | Puerco. | Cenaysian. |
|---|---|---|---|---|---|
| Number of premolars, | ? | 4–3 | 2 | 2–1 | 1 |
| Grooves on premolars, | ? | 7–9 | 11–14 | 12–15 | 14 |
| Molar tubercles: outer; inner; | ? | 4 :2 | 6 :4 | 6 :4 | 9 :6 |

Estimating the geological intervals by dental evolution and faunal succession, there is first the great gap between the Trias of Microlestes and Dromotherium and the Jurassic of the Stonesfield slate; there is a relatively shorter interval, but still a considerable one between this and the Purbeck or Atlantosaurus beds. Then follows another long and very important interval between the Atlantosaurus beds and the Laramie (Upper Cretaceous). The gap between the Laramie and Puerco was relatively short as indicated by the comparatively limited evolution both of the Plagiaulacids and Trituberculates. The Puerco itself was a period in which the Plagiaulacids underwent considerable changes. Then follows an interval which it is most important to fill by future exploration, for between the Puerco and the Wahsatch the differentiation of the even and odd-toed Ungulates must have occurred. The Wahsatch proper does not mark a very extensive evolution of the forms it contains. It passes after a slight break into the base of the Bridger (Wind River) and then begins that splendid and almost uninterrupted succession of lake basins, terminating in the Pliocene. I append a table, to be compared with that published by Marsh in his admirable address of 1877, and exhibit the great progress of the last sixteen years.

The general faunal succession is marked by the sudden appearance and disappearance of certain series and rise and fall of great groups. In the Trias appears the remarkable protodont or primitive-toothed Dromotherium; we cannot determine its order at present. We still have no American fauna corresponding to

THE SUCCESSION OF THE NORTH AMERICAN MAMMALIA.

| PERIODS. | HORIZONS. | CHARACTERISTIC GENERA. | NEW TYPES APPEARING. | TYPES BECOMING EXTINCT. |
|---|---|---|---|---|
| Post or Upper PLIOCENE. | EQUUS. | *Equus*, 5 species. Elephant, *E. primigenius*. Mastodon. Llamas. Camels, *Eschatius*, *Holomeniscus*. Elk, *Alces*. *Platygonus*. Sloths, *Mylodon*, *Glyptodon*. *Ursus*. | | |
| True PLIOCENE. | BLANCO. | *Equus*, 3 species. *Mastodon*, 3 sp. Llamas. *Pliauchenia*. *Platygonus*. Sloth, *Megalonyx*. *Felidae*. (?) *Hyænidæ*. *Mustelidæ*. | | |
| MIOCENE. | LOUP FORK. | *Protohippus*, *Hipparion*. *Mastodon*. Rhinoceroses, *Aphelops*, 5 species. *Canidæ*. *Felidæ*. Rodents. Edentates. Camels and llamas, *Procamelus*, *Protolabis*. Oreodons, 3 genera. Deer, *Blastomeryx*, *Cosoryx*. | | Extinction of Oreodons and hornless rhinoceroses. |
| Upper. | DEEP RIVER. | *Protohippus*, *Anchitherium*. First mastodons. Oreodons, *Cyclopidius*. *Chalicotherium*. Tylopoda. | | Disappearance of Chalicotherium. Ext. of Creodonts, Hyænodons. |
| Middle. | JOHN DAY. | *Miohippus*. Two-horned rhinoceros, *Diceratherium*. *Hyopotamus*. Peccaries. Oreodons. Rodents. *Canidæ*, *Felidæ*. Tylopoda. | | Extinction of Elotheres and Hyopotamus. |
| | WHITE RIVER. PROTOCERAS. (Upper.) | (?) *Miohippus*. Artionyx. | Appearance of tragulines, Elotheres, *Hyopotamus*, pigs and peccaries, true dogs, cats, monkeys. *Leptauchenia*. *Colodon*. *Chalicotherium*. *Aceratherium*. *Protapirus*, *Agriochærus*. Opossums. Tylopoda. *Poëbrotherium*. Creodonts, *Hyænodon*. Rodents. Insectivores. | Extinction of Hyracodons. |
| Lower. | OREODON. (Middle.) | *Mesohippus*. Amynodon. | | Extinction of Amynodons. |
| | TITANOTHERIUM. (Lower.) | *Mesohippus*. Titanotherium. | | Extinction of Titanotheres. |
| | UINTA. | *Epihippus*. *Amynodon*. Titanothere. *Diplacodon*. First Oreodons, *Protoreodon*. First Tylopoda. *Leptotragulus*. Tapirs. Hyracodons. Rodents. Creodonts, *Mesonyx*. | | |
| EOCENE. Upper. | BRIDGER. WASHAKIE. (Upper.) | *Pachynolophus*. Appearance of Amynodons and horned Titanotheres. *Palæosyops*. *Hyrachyus*. *Triplopus*. *Achænodon*. | | Extinction of Dinocerata, of some Creodonts. |
| | BRIDGER. (Middle.) | *Pachynolophus*. Appearance of Insectivora, Cheiroptera, Hyracodons. *Uintatherium*. *Palæosyops*. Creodonts. | | Extinction of Tillodontia. |
| Middle. | WIND RIVER. (Lower.) | *Hyracotherium*. *Palæosyops*. Dinocerata. *Coryphodon*. *Phenacodus*. | | Extinction of Coryphodontia and Condylartha. |
| | WAHSATCH. | *Hyracotherium*. Appearance of Artiodactyls. Perissodactyls: tapirs, horses, titanotheres, lophiodons. First rodents. First coryphodons, lemurs and monkeys. Creodonts, 6 families, *Palæonictis*. | | Extinction of Arctocyons. |
| | Interval. | (*Differentiation of ancient clawed and hoofed placentals*). | | |
| Lower. | PUERCO. | *Ptilodus*, *Neoplagiaulax*, *Polymastodon*. Ancient types of ungulates, carnivores and insectivores: Amblypoda. Condylarthra, Creodonta. Tæniodonta. Tillodontia. Lemurs. | | Extinction of Multituberculates (? Monotremes). |
| | Interval. | (*Differentiation of modern clawed and hoofed placentals*.) | | Disappearance of Marsupials. |
| UPPER CRETACEOUS. | LARAMIE. | *Ptilodus*. *Bolodontidæ* (multituberculates). *Thlæodon*. Trituberculate placentals and marsupials. Typical dentition. | | |
| | Interval. | | | |
| MIDDLE JURASSIC. | Atlantosaurus. | *Ctenacodon*. *Plagiaulax*, *Bolodon*, multituberculates (? monotremes). Triconodonts (? marsupials). Trituberculates (? placentals). Primitive dentition. | | |
| | Interval. | | | |
| UPPER TRIASSIC. | Chatham Coal Beds. | *Protodonta*, *Dromotherium*. *Microconodon*, primitive triconodonts. | | |

the intermediate Stonesfield of England. In the Jurassic Atlantosaurus beds the three supposed representatives of the Monotremes (multituberculates), Marsupials (triconodonts) and Placentals (trituberculates), appear in equal numbers; the latter are generally characterized by the primitive dental formula. In the Laramie the multituberculates continue in great profusion, and the Marsupials and Placentals are also numerous.

The serial succession of the trituberculates from the Mesozoic is still an unknown chapter; we are utterly unable to connect the Dromotheriidæ of the Trias, the Triconodontidæ, Amphitheriidæ and Amblotheriidæ of the Jura with each other, or with any Cretaceous or lower tertiary mammals. The serial relations of the multituberculates, on the other hand, have been made much clearer by the discovery of the Laramie fauna. Cope and Marsh in this country, and Smith Woodward in England, have at last broken into the long barren Cretaceous. In studying the accurate figures published by Marsh and a large collection of teeth recently made for the American Museum by Wortman and Peterson, I find that this Laramie fauna is widely separated from the Jurassic in its general evolution, and as Gaudry, Lemoine and Cope have observed, it approaches more nearly the basal Eocene of the Puerco and the Cernaysian of France. The multituberculates of the Laramie include the Plagiaulacidæ, represented by Ptilodus, the form with two premolars, and Meniscoëssus, with two premolars and crescentic tubercles. Meniscoëssus has a smaller fourth premolar, and is found to lead off to the huge plagiaulacid Polymastodon of the Puerco. The only other multituberculates found are those related to Bolodon of the Jurassic and Chirox of the Puerco. The other mammals of the Laramie range from the mouse to the opossum in size; they have superior molars of the simple tritubercular type—the low cusped or bunodont molar predominating in the upper jaw, and the tuberculo-sectorial in the lower. The dental formula is mostly the typical p. 4. m. 3. Yet, judging by the angular region of the jaws, we have here both Placentals and Marsupials. Some of the teeth remind us strongly of those in the Puerco; their determination, however, is very difficult for the jaws and teeth are almost entirely isolated. From another exposure of the Laramie, Cope has recently found the remarkable type Thalæodon—remarkable because it is highly specialized trituberculate of typical dentition with a jaw which bears

resemblance to that of the multituberculates and of Ornithorhynchus. There is no placental angle nor strong marsupial inflection. This raises the supposition that Thalæodon may be one of the persistent trituberculate Monotremes which we are now looking for.

In the Puerco or basal Eocene, a very marked change occurs, for the American fauna loses some of its cosmopolitan character, the multituberculates or Monotremes die out and the Marsupials are not found at all; in fact they do not reappear in North America until the Miocene.

### ANCIENT AND MODERN PLACENTAL DIFFERENTIATION.

The Puerco is essentially an archaic fauna and is to be regarded as the climax of the first period of placental differentiation, a culmination of the first attempts of nature to establish insectivorous, carnivorous and herbivorous groups. These attempts began in the Cretaceous, and some of the types produced died out in the Puerco, some in the Wahsatch and Bridger; only a few flesh-eaters survived to the Miocene. It is most important to grasp clearly the idea of this functional radiation in all directions of this old Puerco fauna, resulting in forms like the modern insectivores, rodents, bears, dogs and cats, monkeys, sloths, bunodont and selenodont ungulates, and lophodont ungulates. This was an independent radiation of Placentals, like the Australian radiation of Marsupials. What was the cause of the wide-spread extinction of these types? So far as the ancient clawed types are concerned, their teeth and feet seem to be as fully adaptive in many cases as those of the later Unguiculates; the hoofed types were certainly inferior in tooth evolution, for all their molars evolved on the triangular basis instead of the sexitubercular; the most sweeping defect of both the clawed and hoofed types was the apparent incapacity for brain growth, their bodies went on developing while their brain stood still. Thus the stupid giant fauna, the Dinocerata, which rose out of this period, gave way to the small but large-brained modern types. It is noteworthy that the latest survivors of this wreck of ancient life were the large-brained Hyænodons.

Some of the least specialized spurs of this radiation appear to have survived and become the centres of the second or mid-Tertiary radiation from which our modern fauna has evolved. Yet we

have not in a single case succeeded in tracing the direct connection. To sum up, we find on the North American continent evidence of the rise and decline and disappearance of Monotremes and Marsupials, and two great periods of placental radiation, the *ancient radiation* beginning in the Mesozoic, reaching a climax in the Puerco and unknown post-Puerco, and sending its spurs into the higher tertiary, and the *modern radiation* reaching its climax in the Miocene and sending down to us our existing types.

Another Eocene centre was lower South America, which has of late dimmed the prestige of North America in yielding strange forms of life. One theory of this Patagonian fauna is that it was an independent centre of functional radiation like the Puerco and Australian, full of adaptive parallels, but not yielding to Europe or America any of their older types. But Ameghino, to whose energetic researches we are chiefly indebted, believes that he finds a Lower Eocene life zone—a sort of *south polar* centre—which supplied both America and Europe. The Puerco he believes is no older than the Santacruzian which in turn is very much older than the Parana and Pampean formations, which Burmeister has made so well known. This yields the *Homunculus patagonicus* which parallels Cope's Anaptomorphus in presenting a dentition as advanced in reduction as that of man. Ameghino finds here the ancestors of the Macrauchenidæ; he believes the Homolodontotheriidæ are the ancestors of the Chalicotheriidæ—thus deriving a buno-selenodont from a lophodont type; the Proterotheriidæ, he believes, replace the Condylarthra and Hyracotherium in the ancestry of the horses. Similarly the Microbiotheriidæ are the stem of the Creodonts and Carnivores. I cannot coincide with any of these views. The Multituberculates are far older and widely different from the Abderites to which Ameghino traces their ancestry. I fully concur with the opinion of Cope, Zittel, Scott and others that this fauna is of somewhat later age, that it was directly connected with Australia and somewhat later with North America, supplying us, as has always been supposed, with our sloths. I quote from a recent address by Scott:

"The oldest mammals from South America are those from Patagonia, which Ameghino has referred to the Eocene, but which are more probably Oligocene or Miocene. This fauna is of extreme peculiarity and isolation; it is made up chiefly of Edentates, Rodents and Ungulates of those very aberrant types known as Litopterna

and Toxodontia, which are so widely different from the hoofed mammals of the northern hemisphere; together with some primitive forms of Primates, Creodonts and Marsupials. The Marsupials are of extraordinary interest, for they comprise not only forms allied to the opossums, but also to recent Australian forms such as Thylacinus, Dasyurus and Hypsiprymnus. This is a most unexpected fact and seems to point unmistakably to a great southern circumpolar continent."

The Puerco thus remains the most extensively known and productive Lower Eocene centre, yet we have very slender threads of positive evidence to connect its fauna with the later placental radiation.

The Creodonts of Cope occupy the same relation to the modern Insectivores and Carnivores that the Condylarthra do to the ungulates. The American group has been recently enriched by the discoveries of Wortman, and the literature by the careful revision of Scott. This author has divided them into eight families, placing the forms which most resemble the Insectivora in the new family, Oxyclænidae. These families illustrate superbly the same law of functional radiation later repeated in the placental and marsupial Carnivores. The Mesonyx family presents some analogies to the Thylacines. The modern bears are paralleled in the Arctocyons, with their low tubercular molars; Wortman and myself, with fresh materials, have recently added Anacondon to this family, a genus which was doubtfully regarded by Cope as an ancient ungulate. The cats and hyenas are imitated in the Oxyænas and Hyænodons, some of the Miocene forms of which Scott suggests developed aquatic habits; as above noted, some of this family acquired large brains and persisted well in the Miocene. A still more remarkable likeness to the cats is exhibited in the Palæonictis family, which, unlike the Hyænodons, forms its sectorials out of exactly the same teeth as the true cats. The first American Palæonictis was found two years ago by Wortman, and this author and myself have suggested that this may be the long-sought ancestor of the Felidæ. The civets are anticipated in the Proviverridæ; yet both Cope and Scott, the highest authorities on this subject, believe that the dog-like Miacidæ alone formed the connecting link between the Creodonta and the true Carnivora.

The foot structure of the ancient Puerco ungulates is still only

partly known. Cope has divided these animals into the Amblypoda and Condylarthra. The Amblypoda are represented in the Puerco by a large form called Pantolambda, with selenodont triangular upper molars and possibly by Periptychus, with bunodont triangular molars. The Pantolambda molars were, as Cope has shown, converted into those of Coryphodon, the great lophodont Amblypod of the Wahsatch, by a process exactly analogous to that in which the *anterior* half of a Palæotherium molar was formed, that is, they acquired outer and anterior crests but no posterior crests. This Coryphodon molar type was still later converted into the Uintatherium type of swinging around the outer crests into a transverse crest. I have recently made a careful study of the fore and hind feet of Coryphodon, and have found that while the fore foot was subdigitigrade like that of the elephant, the hind foot was fully plantigrade, the entire sole resting upon the ground. The relation or connection between the Bridger Dinocerata and these earlier Amblypoda is still unknown. The Puerco Periptychus left no descendants. The other ungulates of the Puerco were the Condylarthra, the primitive Phenacodontidæ, the supposed ancestors of the artiodactyls and Perissodactyls. Much remains to be done to clear up this question.

*Succession of the Perissodactyls.*

In the Wahsatch and Wind River we find not only the last of the Phenacodonts and Coryphodonts and the first of the Dinocerata, but the first of the true Artiodactyls and Perrissodactyls. Recent studies of Cope, Schlosser, Pavlow, Filhol have been directed to the phylogeny of the Perissodactyls with very different conclusions. I agree most closely with Schlosser, and have endeavored to show that the molar teeth give us a key to their natural arrangement as shown in this column.

. . . { Titanotheres.

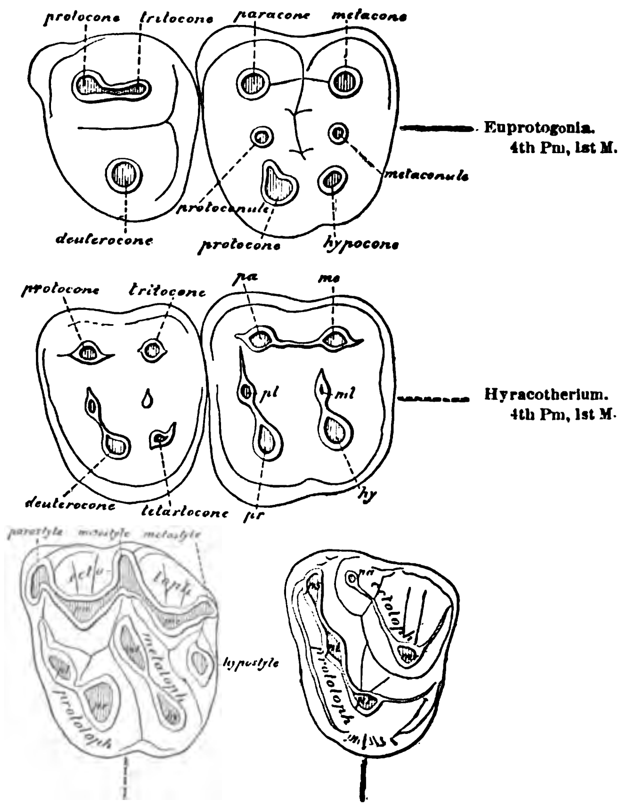
. . . { Horses. Palæotheres.

. . . { Tapirs. Lophiodonts. (Helaletes).

. { Hyracodonts. Amynodonts. Rhinoceroses.

Upon one side the Titanotheres present the selno-bunodont extreme with most analogies to the Artiodactyla in tooth structure and in their truly artiodactyl fore feet and bony horns. (If, as Cope supposes, the Diplartha form a natural group, some perissodactyls should certainly be more artiodactyla than others.) The horses and Palæotheres diverge

from the buno-selenodont type toward the lophiodont; they were early separated in foot structure. The tapirs, lophiodonts and Helaletes show well-marked transverse crests and incipient external crests. This brings us to the other lophiodont extreme, the rhinoceros-like forms, with complete transverse and external crests. There are many other minor characters which support this as the natural arrangement of the Perissodactyls. I think it




Homologies in the Horse and Coryphodon molars and premolars.

can be shown conclusively that these eight or nine series diverged from each other before the Wahsatch, and that all attempts to derive them from each other in later periods will break down. They will be found to converge into the unknown sub-Wahsatch period, to stem forms indicated by the brackets.

One of the most decided reforms in the matter of classification is the use of the *family* division. Pre-darwinian writers considered animals as arranged in circles; post-darwinian writers all regard them as in vertical lines, giving off side branches. Classification should keep pace with phylogeny in paleontology. Yet there are two clearly defined schools of classification to-day. The one, led by Flower, Cope, and Lydekker, practically adheres to the old circular system; according to this, comprehensive families are formed out of members of different lines of descent which *happen to be in the same stage of evolution;* for example, among the Ungulates, a horse in the first stage of its evolution is called a lophiodont (*i.e.*, it is placed in the *Lophiodontidæ*) in the next stage it is called a palæothere (*i. e.*, in the *Palæotheriidæ*). The extreme application of this method by Cope has led to a total misunderstanding abroad of his real phylogenetic views. The other school, including Schlosser, Scott, Zittel, and myself, adopt the vertical system, according to which a horse is called a horse, a tapir a tapir, a rhinoceros a rhinoceros, from the moment when they clearly appear as such. I have attempted elsewhere to show that the circular system is confusing, that it ignores the divergence of structure which resulted from thousands of years of physiological isolation; that finally it is only possible when we define families upon the false system of single characters. In England and France the adherence to the circular system is largely due to traditional reverence for the Lophiodontidæ, which has become an *omnium gatherum* for early odd-toed Ungulates—just as were the Pachyderms of Cuvier until Owen proved that that term had no meaning. To-day, no one can say exactly what the Lophiodon itself was; it appears to have been an aberrant and early extinguished line. In the vertical system the great stages of evolution may be indicated by sub-family divisions, as in the following table. It is practically the same system as that which Flower applies to the existing mammals.

All the tendency of recent discovery has been to show that these lines are separate as far back as we can now trace them, that is, to the Wahsatch or Suessonian. Such being the case, we are no more justified in placing the ancient tapirs and horses in one family (Lophiodontidæ) than the modern.

In the matter of genera, opinion divides on different lines. Flower and Lydekker place all the extinct and modern rhinoc-

eroses (except Elasmotherium) in one genus, while Cope divides the rhinoceroses into a large number of genera. Here we are dealing not with great separate lines of descent but with *stages* of evolution in the same or a few closely parallel lines. If we unite a large number of stages into one genus of rhinoceroses, to be

| BASAL EOCENE. Puerco to Wahsatch. | MIDDLE AND UPPER EOCENE. Premolars simpler than molars. Wahsatch to Utina. | MIOCENE. Premolars transforming into molars; or Pm = M. White River to Loup Fork. | PLIOCENE. Pm = M. |
| --- | --- | --- | --- |
| | Palæosyopinæ | Titanotherinæ | = Titanotheriidæ |
| | Hyracotherinæ | Anchitherinæ....Equinæ | = Equidæ |
| | Palaplotherinæ | O } | = Palæotheriidæ |
| | Palæotherinæ | O } | |
| | Systemodontinæ | Tapirinæ | = Tapiridæ |
| | Helaletinae | Colodontinæ | = Helaletidæ |
| | Lophiodontinæ | O | = Lophiodontidæ |
| | Hyrachyinae | Hyracodontinæ | = Hyracodontidæ |
| | Amynodonthinæ | Metamynodontinæ | = Amynodontidæ |
| | | Aceratherinæ...Rhinocerotinæ } | = Rhinocerotidæ |
| | | Diceratherinæ.. Elasmotherinæ } | |

Family series converging into Condylarthra or Stem Perissodactyla.

consistent we should do the same with the horses and tapirs. Nevertheless, it is very difficult, if not impossible, to agree as to what shall constitute a generic stage.

The Titanotheres have been traced by Cope back to Lambdotherium in the Wahsatch; in the Wind River the true Paleosyops is found, and in the Bridger this becomes the predominant perissodactyl family, and spreads out into a great variety of forms,

which have recently been carefully described by Earle. In the Washakie there are some still larger forms, and Marsh has traced the line through the teeth of Diplacodon of the Uinta to the true Titanotheres. Still the origin of the flattened skull and remarkable anterior pair of horns has never been known; Hatcher reports species with very small horns in the base of the Titanotherium beds (Lower Miocene). Wortman has just reported to me the brilliant discovery of an Upper Eocene (Washakie) Paleosyops with a flattened skull and rudimentary horns just appearing upon the nasals. This forms the desired connecting link.

The early history of the horses, probably starting with the Puerco Condylarth Euprotogonia, and passing through Hyracotherium, Pachynolophus, Epihippus, Mesohippus, is now familiar enough. It is the later history which requires elucidation, and is producing the most unexpected number of parallel lines of horses, out of one of which only our modern horse sprang. Here we are especially indebted to Cope, Pavlow, and Scott. By general consent Hipparion comes out of its old position in the true line as displaying the most extreme variations in the crowns of the molar teeth in compensation for the backward evolution of its feet. Scott has been especially investigating the Upper Miocene horses; I quote from the MSS. he has kindly lent me, in which he proposes to remove also the classical Anchitherium of Cuvier. He says: "The American genera, Mesohippus and Miohippus may confidently be regarded as important members of the equine stem, while Anchitherium (of Europe) from present information would appear to belong to an abortive side branch leading to no permanent results." Scott has also discovered an important intermediate form linking Miohippus with Protohippus.

The Paleotheres have not been found in America.

The tapir line has been traced by Cope and myself back to Systemodon of the Wahsatch, and Isectolophus of the Bridger and Uinta. These forms have simple premolars, but bear the most striking resemblance to the tapirs in the molars both above and below. All previous attempts to determine the Miocene representatives of the tapirs have been erroneous. Wortman and Earle have just published an account of two Lower Miocene species of true tapirs, which, both in foot and tooth structure, definitely carry the American tapir line up to the Middle Miocene, where it is again lost sight of. These species belong to the genus

Protapirus, which Filhol has found in the Oligocene of France, thus adding an important geological parallel. The Wahsatch tapirs were a little larger than the horses or Hyracotheres which were about the size of a fox, and much smaller than the ancestral Titanotheres. Another family of small, slender perissodactyls were most nearly allied to the Lophiodons of Europe of any American forms.

These are the Helaletidæ distinguished by feet tending to monodactylism and narrow hoofs like those of the deer; even in the Wahsatch Heptodon the lateral toes are quite short and raised off the ground. The molars, like those of the Lophiodons of Europe, are intermediate between those of the tapir and the rhinoceros, but both teeth and feet preclude our uniting these forms with either the tapirs or with the Hyrachyus family, as Cope has done. The Bridger successor is Helaletes, which Marsh mistakenly supposed was an ancestral tapir, and the integrity of this line is now firmly established by the discovery of the Miocene Colodon. This is described by Marsh as a successor of Helaletes, and Wortman and Earle have just published an account of the teeth and feet, showing that Colodon is widely separated from the contemporary tapirs, and is the last member of the Heptodon-Helaletes line.

The rhinoceroses of America comprised the true Aceratheriinæ and Diceratheriinæ, and what may be called the pseudo-rhinoceroses, the Hyracodons and Amynodons; all these forms present the true rhinoceros molar pattern, but they diverge most widely in the structure of the anterior teeth and of the feet. The Hyracodons first appear in the numerous and diversified Hyrachyus of the Bridger, some of which exhibited rudimentary horns upon the back part of the nasals (Colonoceras); they retained a full set of equal-sized incisors and canines, and acquired a horse type of skull, skeleton and locomotion. Scott has well named them the "cursorial rhinoceroses." Colonoceras probably did not, as Marsh has suggested, branch off into Diceratherium, for the horns of this true rhinoceros are developed at the ends of the nasals; the Hyrachyinæ sent off as a side branch the deer-like Triplopus of the Washakie, and terminated in the Hyracodons of the Lower Miocene.

The Amynodons, at the time of their discovery by Marsh, were naturally supposed to be the long-sought Eocene rhinoceroses, but I have shown that no Amynodon can fill this rôle. Garman's

discovery of the skull of the remarkable Miocene Metamynodon tended to confirm my views, and I have now to report the discovery of many skulls and a nearly complete skeleton by the American Museum expedition This proves that the Amynodontidæ were remarkable side forms. In wide contrast with the true rhinoceroses, the upper and lower canines develop into huge, partly recurved cusps, like those of the boar. As in Elasmotherium, the premolars become greatly reduced, and the molars tend to hypsodontism. The lower molars are long and narrow, like those of the anomalous Cadurcotherium of the Oligocene of Europe—it is thus rendered probable that Cadurcotherium is not a sloth, as Filhol has suggested, but is an aberrant rhinoceros, related to, if not identical with, the Amynodons. The hypsodontism in some Metamynodon teeth is accompanied by a partial loss of enamel. To complete the aberrant character of this family, we find that it has four equal-sized and completely functional toes in the fore foot, like those of the Titanotheres, not with the fifth toe reduced as in the contemporary Aceratheria.

The true rhinoceroses, we remember, are distinguished by the entire loss of upper canines. Wortman has just reported finding rudimentary upper canines in both the milk and permanent dentitions of the older Miocene species. The true rhinoceroses suddenly appear in the Lower Miocene of America and Oligocene of Europe; we have not yet traced them back. In a collection of Lower Miocene skulls recently obtained for the American Museum we find that the premolars are still very simple. In the higher Oreodon beds all traces of the superior canine are lost, and the premolars have become more like the molars. As the origin of the rhinoceroses still remains a mystery, so their late evolution needs clearing up. The American series suddenly terminate in the huge, hornless forms of the Upper Miocene. I find there is still no unanimity of opinion in Europe as to the phyletic relationships of the Miocene, Pliocene and existing species.

### *Succession of the Artiodactyls.*

The Eocene Artiodactyl phylogeny is still far behind that of the Perissodactyls, but the Miocene and Pliocene succession has been worked up with great success and clearness by Cope and Scott. The latter says in a recent paper: "All the great groups of Artiodactyla are seen to arise independently from the Buno-sele-

nodonta which forms as it were a lake, from which several streams, flowing partly in parallel, partly in divergent directions, are derived."

The Elotheriidæ appear in Parahyus of the Bridger and Achænodon of the Washakie, and terminate in the Middle Miocene in the gigantic *Elotherium ramosum*, an animal with a skull three feet long, both the jaws and skull being armed with long branching processes. The true bunodont pigs and peccaries have not yet been found lower than the White River.

Scott has traced the Oreodons back to Protoreodon of the top of the Eocene. The aberrant Agriochœridæ, he believes, were doubtfully connected with the true Oreodons by a lower Eocene stem form. The true Oreodons, which existed in great herds in the Lower Miocene, have been divided by Cope and Scott into three parallel lines extending into the Loup Fork, namely, the large Merycochœrus, the medium-sized and more primitive Merychius and the small, highly-specialized Pithecistes.

The Tragulines are represented by Leptomeryx, Hypertragulus and Hypisodus. Leptomeryx is believed to be a side member of the main family. Here I may speak of the recent discovery of the characters of the Protoceratidæ, a new family with a remarkable ensemble of characters.

In 1891 Marsh described the female skull of Protoceras with a small pair of parietal protuberances. The male skull was found in 1892. It is armed not only by upper canine tusks, but by four pairs of cranial protuberances, two of which might be dignified by the name of osseous horns; it thus presents the armature of an Uintatherium upon a small scale. Besides parietal and two pairs of frontal protuberances, there are a pair of most exceptional maxilliary plates. The fore foot is like that of Tragulus, while the hind foot is didactyl like the deer. We can at present form no idea of its affinities.

The oldest American Artiodactyl certainly known is the tritubercular Pantolestes of the Wahsatch. Cope believes the line of American llamas may have sprung from this, and have been continued through Homacodon of the Bridger. The first undoubted cameloid is Leptotragulus of the Uinta, a comparatively recent discovery. It has strikingly reduced feet for such an early form. Poëbrotherium of the White River and John Day has quite the proportions of the living llama; thence the line passes into

Protolabis of the Deep River and John Day. Scott believes that these forms are undoubtedly related to both the camels and llamas, and that in the Loup Fork, perhaps in the two species of Procamelus, the division occurs, *P. Angustidens* passing into the camels, and *P. occidentalis* into the llamas. The Pliocene Homocamelus, Holomeniscus and Eschatius, Scott believes may represent a highly specialized side line of camels; while Pliauchenia, still imperfectly known, may belong on the llama side. The deer represented by Cosoryx and Blastomeryx are, so far as we know, not of American origin, for they first appear in the Upper Miocene at Loup Fork.

### *The Ancylopoda.*

The order Ancylopoda Cope presents the most signal exception to the law of correlation. It is only quite recently that Filhol, Forsyth, Major and Depéret have brought together the sloth-like phalanges with the ungulate type of teeth of the Chalicotheriidæ. Since 1825, when Cuvier described the phalanges from Eppelsheim as those of a "*pangolin gigantesque*," referring to their deep clefts, and 1833, when Kaup named the teeth, these structures were always considered distinct. It is probable that Moropus and other supposed sloths described by Marsh from our Miocene also belong in this exceptional order. As now restored by Filhol and myself, this remarkable Chalicotherium had a gait less clumsy than the sloth, and something between a huge cat and a hoofed animal; it combined the skull of a primitive ungulate with the molars of an Eocene Titanothere, for the premolars are simple. The limbs, wrist and ankle bones are chiefly ungulate and perissodactyl. In viewing this combination of characters, the first question to settle is which set of characters is secondary and adaptive. I agree with Depéret, as against Filhol who regards this as an aberrant Edentate, that the unguiculate characters are secondary; but I do not believe it is very near the Perissodactyla. It seems to have sprung rather from the primitive Ungulate stem before it had parted with its unguiculate characters. Perhaps it came off from the Wahsatch Meniscotherium, a member of the Condylarthra, which it very closely resembles in its skull and molar structure and in its dental curve. Marsh, by the way, has just added to our knowledge of this little Wahsatch genus by describing its fore and hind feet, which are more primitive than those of Phenacodus or Hyrax.

While the Creodonta were imitating all modern carnivores, is it not possible that the Condylarthra gave off a sloth-like form for fossorial and semi-arboreal habits?

Last summer while this problem was being discussed, we were brought to face with the exact counterpart of Chalicotherium which may be called a *clawed odd-toed* form, by the surprising discovery of a hind foot, which represents a *clawed even-toed* animal. This was found by the American Museum party in the Protoceras beds of South Dakota, and has been named Artionyx. This foot has a truly artiodactyl tarsus and metatarsus like that of the pigs or oreodons. Yet it possesses five toes terminating in large uncleft claws. It has been suggested by Wortman and myself that it represents an Artionychine (even-clawed) division and that Chalicotherium represents a Perissonychine (odd-clawed) division of the Ancylopoda; in other words, that a double parallelism exists with the Ungulata. Another explanation may be that these genera are highly specialized Artiodactyla and Perissodactyla respectively; Scott has made the ingenious suggestion, tending to support this theory, that the Artionyx foot is the long unknown foot of the aberrant oreodont Agriochœrus of Leidy. This summer will probably determine the truth of this suggestion, for two parties are hunting in the beds in which Agriochœrus and Artionyx occur.

Thus an immense number of problems still await solution, and demand the generous coöperation of European and American specialists in the use of similar methods of research, in the prompt publication of descriptions and figures, and in the free use of museum collections. I may be pardoned for calling general attention to the service which the paleontological department of the American Musuem is trying to render in the immediate publication of stratigraphical and descriptive tables of western horizons and localities.

### THE FACTORS OF EVOLUTION.

A few words in conclusion upon the impressions which a study of the rise of the mammalia gives as to the factors of organic evolution. I refer also to recent papers by Cope, Scott and myself.

The evolution of a family like the Titanotheres presents an uninterrupted march in one direction. While apparently prosperous

and attaining a great size, it was really passing into a great corral of inadaptation to the grasses which were introduced in the Middle Miocene. So with other families and lesser lines, extinction came in at the end of a term of development and high specialization. With other families no causes for extinction can be assigned, as in the lopping off of the smaller Miocene Perissodactyls. The point is that a certain trend of development is taken leading to an adaptive or inadaptive final issue—but extinction or survival of the fittest seems to exert little influence *en route*.

The changes *en route* lead us to believe either in predestination —a kind of internal perfecting tendency, or in kinetogenesis. For the trend of evolution is not the happy resultant of many trials, but is heralded in structures of the same form all the world over and in age after age, by similar minute changes advancing irresistibly from inutility to utility. It is an absolutely definite and lawful progression. The infinite number of contemporary developing, degenerating and stationary characters preclude the possibility of fortuity. There is some law introducing and regulating each of these variations, as in the variations of individual growth.

The limits of variation seem to lie partly in what I have called the "potential of evolution." As the oösperm or fertilized ovum is the potential adult, so the Eocene molar is the potential Miocene molar. We have seen that the variations of the horse and rhinoceros molars, apparently so diverse, are really uniform,—is not this evidence that the stem perissodactyl had these variations *potentially*, waiting to be called forth by certain stimuli? This capacity of similar development under certain stimuli is part of the law of mammalian evolution, but this does not decide the crucial point whether the stimulus is spontaneous in the germ or inherited from the parent. I incline to the latter opinion.

# PAPERS READ.

NOTES ON ASPIDIOTUS PERNICIOSUS. By Prof. C. V. RILEY, Dept. of Agriculture, Washington, D. C.

[ABSTRACT.]

*Aspidiotus perniciosus* (Comst.) is a very destructive coccid to fruit and other trees in California; but has not occurred hitherto in the Atlantic states. The paper records its recent discovery at Charlottesville, Va.; discusses the urgency and means of eradicating it while yet in limited confines and considers the question of state as against federal action which such matters involve.

ERASTRIA SCITULA, A VALUABLE INSECT TO INTRODUCE TO AMERICA. By Prof. C. V. RILEY, Dept. of Agriculture, Washington, D. C.

[ABSTRACT.]

*Erastria scitula* is a noctuid of predaceous, or partially parasitic habit, living on and destroying *Lecanium oleæ* in south France. It breeds rapidly and is a most effective destroyer of the coccid. *Lecanium oleæ*, under the name of the black scale, is one of the worst insect enemies of the fruit-grower, especially the olive-growers of southern California. The author urges the importance of the introduction of the *Erastria*, which he hopes to effect with the aid of H. Rouzaud of Montpellier, who has particularly studied the species.

SPHIDA, A MYTH. By Prof. C. V. RILEY, Dept. of Agriculture, Washington, D. C.

[ABSTRACT.]

THE paper gives the life-history of an interesting noctuid, whose larva is sub-aquatic, and has exceptionally developed and adaptive anal spiracles. It produces two annual generations, with such differences between the imagos of the two, as to have caused them to be described as specifically distinct. Grote erected the genus Sphida for this species, on the mucronate clypeus, separating it from Arzama; but the type of *Arzama densa* Walker, which Riley examined in 1886, has the mucronate clypeus. Prof. J. B. Smith has also shown that *Bellura gortynoides* Walker is the same insect and has priority over the other synonyms.

SEAT OF LIFE IN THE HOUSE-FLY. By Dr. JOHN B. SMITH, New Brunswick, N. J.

[ABSTRACT.]

THE house-fly has been observed to be capable of standing a good deal of mutilation without manifesting pain and without dying at once. To test the question as to what injury would kill immediately, the following experiments were made:

Flies, when decapitated, lived ten to sixteen hours and would run or fly when disturbed; but had lost all idea of direction. If undisturbed they rested quietly, the forelegs occasionallv passing over the point where the head should have been. The parts of the head retained no life.

When the abdomen was cut off, flies lived from six to ten hours, and for the greater part of the time were active, running and flying readily. There was at no time any interference with the power of motion, and the only sign of an unusual nature was the frequent extension or withdrawal of the tongue.

When head and abdomen were both removed, these parts died in a few minutes, but the thorax retained life more than six hours and could be readily induced to walk and sometimes to make use of the wings.

When abdomen and that part of the thorax bearing the hind legs was removed, the latter died almost immediately, while head, pro- and meso-thorax lived for nearly six hours, and all of the appendages responded readily to stimulation.

Cutting off the head, in addition to the foregoing, makes no perceptible difference in the length of life of the thoracic parts.

Dividing an insect between the first and second pairs of legs, killed all the posterior parts at once, while the anterior parts remained alive from four to five hours, the forelegs and head parts responding readily to touch, while the tongue was frequently extended voluntarily.

When the head was also removed, the prothorax with the fore legs lived three hours.

Cutting an insect through the prothorax, just above the fore legs, killed it at once.

Cutting the dorsal vessel, or heart, did not seem to distress the flies so treated, and they lived twenty-four hours in confinement—quite as long as uninjured specimens introduced as a check.

Flies, in which the heart and alimentary canal were cut, lived twelve hours or more.

Cutting the nervous cord behind the posterior legs, paralyzed those members, but affected neither wings nor other legs.

Cutting the cord between the middle and hind legs produced exactly the same effects.

Cutting the cord between the fore and middle legs, and close to the latter, paralyzed everything behind the fore legs, as well as the wings, the head and fore legs remaining alive six hours thereafter.

The conclusion is that the vital point in a fly lies in the large ganglion

of the nervous system, which is situated in the prothorax, and that this alone, though separated from all other ganglia, is capable of maintaining life in the parts supplied by it, while no other ganglion alone is able to maintain even the semblance of life, death ensuing almost immediately upon injury to the prothoracic ganglion.

[Printed in Science for Oct. 13, 1893.]

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THE RESPIRATORY MECHANISM OF THE LAMPREY (PETROMYZON). By Prof. S. H. GAGE, Ithaca, N. Y.

[ABSTRACT.]

1. THE lamprey has a purely aquatic respiration, and as it must frequently make great exertion, the gills offer a great surface for the inhaled water.

2. In order that the most complete oxygenation of the blood shall be secured, the water must be respired but once, and to ensure this the branchial apparatus is supplied with a complicated system of valves, which cause the exhaled water to be sent so far backward that there is no risk of re-inspiring it.

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THE CORRELATION OF STRUCTURE AND HOST-HABIT WITH THE ENCYRTINÆ. By L. O. HOWARD, Dept. of Agriculture, Washington, D. C.

[ABSTRACT.]

THE paper consists of a review of the host-insects affected by the several genera of the Chalcidid sub-family Encyrtinæ, most of the instances being new. A study is thus made of the relation between these host-insects by groups and of the parasites by genera, showing a nearly complete correspondence according to the present accepted classification of the latter. Then, after a careful consideration of the characters used in classification, thirteen new genera are established from the type genus Encyrtus, and it is shown that with this change the correlation becomes perfect. Thus we have once more an exemplification of the axiom that structure is dependent upon habit. It is shown that a knowledge of the important biological features may be an important aid to a discussion as to the relative value of morphological characters, and the importance of similar work in other groups of parasites is pointed out.

[Printed in The Wilder Quarter Century Book.]

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THE MAMMALS OF THE UPPER CRETACEOUS. By Prof. HENRY F. OSBORN, Columbia College, New York.

[ABSTRACT.]

THIS paper describes a collection of four hundred specimens of teeth and jaws belonging to the small cretaceous mammals. It definitely deter-

mines this fauna as much more limited in variety and quite different in evolution and affinities from what Professor Marsh has led us to believe.

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THE PRODUCTION OF RACES AND VARIETIES OF BACTERIA IN MIXED CULTURES. By Dr. THEOBALD SMITH, Dept. of Agriculture (Bureau of Animal Industry), Washington, D. C.

[ABSTRACT.]

IN January of 1891, I discovered that two species of Bacteria, which were accidentally growing in the same culture, had been quite markedly modified by such associated growth. The pathogenic species (*Bac. choleræ suis*), had been considerably attenuated in virulence, while the associated saprophytic species (*Proteus vulgaris*) was now made up of three varieties, possessing different degrees of peptonizing power and corresponding to three species of *Proteus*, described by Hauser in 1885. A repetition of the experiment led to the same result. The importance of this observation of the mutual action of different species of bacteria, foreshadowed but not demonstrated in the works of former observers, need not be insisted on, for its possible bearing on the rise and decline of epidemics of human and animal diseases is at once apparent.

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A NEW SPOROZOON IN THE INTESTINAL VILLI OF CATTLE. By Dr. THEOBALD SMITH, Dept. of Agriculture (Bureau of Animal Industry), Washington, D. C.

[ABSTRACT.]

THE paper consists mainly in the description of the peculiar morphology of this micro-organism.

[Printed in Bulletin No. 3 of Bureau of Animal Industry, p. 73.]

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THE INSECT GUESTS OF THE FLORIDA LAND TORTOISE. By HENRY G. HUBBARD, 101 Griswold Street, Detroit, Mich.

[ABSTRACT.]

AN account of the discovery of a subterranean fauna connected with the land tortoise *Gopherus polyphemus*.

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A COMPARATIVE STUDY OF THE LUNG WITH SPECIAL REFERENCE TO THE COMMUNICATION OF ONE AIR-SAC WITH ANOTHER. By Dr. W. S. MILLER, University of Wisconsin, Madison, Wis.

[ABSTRACT.]

THE paper is a general consideration of the structure of the lung from the lowest form, as found in Necturus, up through the different types to

man; showing how the lung becomes more complex, and at the same time that no communications are found between the air-sacs.

[The paper was illustrated by a complete set of specimens showing the types; also by several reconstruction models of the mammalian lung.]

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REPORT OF THE COMMITTEE ON AN AMERICAN TABLE AT THE NAPLES ZOÖLOGICAL STATION.

MADISON, WIS., AUGUST 17, 1893.

*To the Biological Section, A. A. A. S.:*

The Committee on the American Table at the Naples Station takes great pleasure in reporting that the Smithsonian Institution has assumed the entire support of the table for three years. On account of this generosity on the part of the Smithsonian Institution your Committee did not find it necessary to use the money ($100) appropriated by the A. A. A. S. in 1892, towards the support of said table.

Respectfully submitted,

C. W. STILES,

*Chairman of Committee.*

## SECTION G.

# BOTANY.

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## ADDRESS

BY

# CHARLES E. BESSEY,

VICE PRESIDENT, SECTION G.

## *EVOLUTION AND CLASSIFICATION.*

MEMBERS AND FELLOWS OF THE SECTION OF BOTANY:

IT is fitting in this address, which opens the work of the new section of botany, when for the first time in the history of the Association we meet officially as a body of botanists, that I should give voice to thoughts which have come to many of us during these later years. As we have gathered up the scattered masses of botanical knowledge, laboriously wrought out by many isolated workers, and attempted to fit them together into a consistent whole, which should outline the structure of the temple Botany, we have found that the workmen have not always followed the same architectural plan, and have often used different units of measurement. With the increasing specialization so noticeable year by year there is a corresponding lack of coördination of work. To this lack of coördination, this want of unity of measurement, this misunderstanding of plan, we can no longer close our eyes, and I therefore feel free to invite your attention to the following somewhat summary discussion of the causes of the present unsatisfactory condition, in the hope that we may thereby be enabled to see how we may make some improvement.

All botanical knowledge finally culminates in some kind of classification. The work of the morphologist and the physiologist no less than that of the professed systematist all find their final expression in classification. The idea which has sometimes found

favor among botanical workers, that there are some departments of botany which have nothing whatever to do with classification, is entirely erroneous. It may be true that a particular worker may make no such use of his results, but it is true nevertheless, that his results will be used and given place and meaning in the great structure which we know as the System of Plants. We must not overestimate the value of the work done in particular fields. No one to-day will deny the importance of the work of the histologist who constantly surprises and astonishes us with the depth of insight which he attains of the minute structure of the cell. We follow the morphologist as he skillfully traces the homologies of organs in related groups of plants, and we watch with delight the cunningly devised experiments of the physiologist whereby he is enabled to penetrate further and further the mystery of plant life. The facts of histology, morphology and physiology are of great biological importance, but the greatest of all biological facts is that the world is peopled with living things. We may group and arrange in orderly sequence the histological facts of the science: we may do likewise with the facts which the morphologist has discovered: we may make a classification of all the known physiological facts, but beyond and above these lies the greatest grouping of all, the grouping in orderly sequence of the organisms themselves whose histology, morphology and physiology we have studied. Do not misunderstand me. When I claim the first place in importance for the classifiacation of living things, do not for a moment suppose that this implies first in point of time in the study. Because the superstructure is the culmination of the building, we do not necessarily begin with it and leave the foundation until later.

But all this is preliminary, and I have referred to it merely to be sure that we are fully agreed as to certain fundamentals.

It is now a full third of a century since a great light was first turned upon all biological problems by the formulation of the doctrine of evolution by the master mind of Darwin. In its light many puzzles have been solved, and many facts hitherto inexplicable have been made plain. We now know what relationship means, and we have given a fuller meaning to the natural system of classification. From the new point of view a natural classification is not merely an orderly arrangement of similar organisms. It is an expression of genetic relationship. The present similarity of two organisms is not enough to determine their relationship, or place in a system.

Common origin must be inferred in order that relationship shall be assumed.

Furthermore, in the light of evolution we now see the meaning of many reduced structures whose significance was formerly not at all or but vaguely understood, The rudimentary stamens of the Labiatæ, and Scrophulariaceæ, the rudimentary ovules of some Compositæ and Gramineæ, the rudimentary calyx of most Umbelliferæ and many Compositæ are no longer difficult to understand. We have become familiar with the fact that degradation is a prominent factor in the vegetable kingdom. Evolution has by no means always involved an advance in structural complexity. Even in those cases where the organism itself has advanced, frequently some of its organs have undergone degeneration. Still more frequently the whole organism has suffered structural degradation, and we commonly assign it a lower place. Often this catagenesis is a result of parasitism or saprophytism, as is so well illustrated in the " fungi," where the degradation has gone so far that their relationship has to a great degree been obscured. Among flowering plants we have numerous instances of degradation through parasitism, as, for example, the species of Cuscuta, the Orobanchaceæ, Loranthaceæ, Rafflesiaceæ, Balanophoraceæ, etc. In the dodder we have no difficulty in recognizing a degenerated morning glory; the broom-rape is apparently a degenerated figwort, and the relationship of the mistletoe to the Santalaceæ is equally plain. The Rafflesias and the Balanophorads have suffered such great degradation as to almost totally obscure their affinities.

But there are also many cases of a catagenesis not due to a dependent habit, in which we have evidence of simplification from a more complex structure. Thus in the willows and poplars where we have a raceme of very simple flowers, each consisting of a single ovary, or one to many stamens, it is readily seen that this simplicity is not primitive. The ovaries are not single carpels, but are composed of two or three united. The flower of the willow is simple by a degeneration from a higher type, probably a tricarpellary or penta-carpellary type, by the loss of its floral envelopes and stamens or pistils. Puzzling as the question of the relationship of the willows has been to systematists, it is no longer difficult to recognize in them, when viewed in the light of evolution, apetalous, declinous Tamariscineæ.

The knotworts, amaranths, chenopods, and polygonums are

likewise simplified forms of well known higher types. Every botanist has recognized the close relationship of the knotworts (Illecebraceæ) to the pinks (Caryophyllaceæ) from which they differ mainly in being apetalous. So close indeed is the relationship between these two groups that in earlier editions of Gray's Manual they were regarded as one, and it was only with violence that they were taken asunder. The amaranths (Amarantaceæ) evidently represent a similar scarious and apetalous modification of a higher type allied to the pinkwort Family. In the chenopods (Chenopodiaceæ) there is a similar apetalous, but not scarious reduction in the perianth, and in other characters they are so much like the amaranths that they are always regarded as closely related. In the polygonums (Polygonaceæ) the simple little flowers are so only by reduction from a higher type. Here the flowers have suffered little if any more reduction from the caryophylline structure than have the knotworts (Illecebraceæ), and like the latter, are apetalous, with a one celled, one-ovuled, pluricarpellary ovary. The polygonums should never have been separated from their relatives the pinks, simply because they have undergone such comparatively slight modifications in structure.

It would be easy to multiply examples like the foregoing not only among the Dicotyledons, but also among the Monocotyledons, where the rushes, aroids, palms, sedges and grasses are successively greater reductions and simplifications of the lily type. It is, however, unnecessary to prolong this part of the discussion. Every naturalist should be as familiar with these illustrations of evolution by simplification as he is with those of evolution by complication. In the growth of the great tree of life, while the development has been most largely in an upward direction so that the great body of the tree has risen far above its point of beginning, there are yet multitudes of twigs and branchlets which droop downward.

In the attempts to arrange in orderly sequence the vast assemblage of living things which to-day people the earth we must constantly keep in mind the fact that they have been evolved from preexisting forms. I need not now, before a body of scientific men, speak of evolution as an hypothesis: for we know it as a great biological fact, as to the existence of which there is no shadow of doubt. A natural classification will conform strictly to the lines of evolution, it will be in fact a clear exposition of the successive steps in its progress. In such a classification the primitive forms will pre-

cede the derived ones, and the relation of the latter will be positively indicated. Moreover, in such a system, there will be no confusion between the primitively simple forms and those which are so by derivation.

An examination of our common systems shows them sadly deficient in the essentials of a scientific classification. This is particularly true of the treatment of the flowering plants, at the hands of English and American botanists. We are all familiar with the usual sequence of the families, first the Dicotyledons, followed by the Monocotyledons. In the former we have in succession the Polypetalæ, Gamopetalæ and Apetalæ, the particular sequence being in Polypetalæ from those with several, separate and superior ovaries to those with one compound inferior ovary, while in the Gamopetalæ we begin with those with an inferior much modified ovary, and proceed to those with a superior less modified ovary. In the Apetalæ we find first the plants having a superior ovary, than those in which it is inferior, and we finally reach a confused aggregation of families in which some have the ovary inferior, while others have it superior. When this sequence was first outlined the Ranunculaceæ and their relatives were regarded as representing the highest type of the Dicotyledons, and the series was therefore a descending one, but this was so obviously an error that without any material change in the sequence a new interpretation was given it. We now ascend from the Ranunculaceæ, through the Polypetalæ, and continue the ascent until we reach the Compositæ, after which we begin a descent to the Labiatæ, thence passing into the Apetalæ we rise for a distance, and then descend again to the Amentaceæ. Nothing more unnatural could have been readily devised to represent the Dicotyledons than this undulating series, over whose serpentine path the young botanist wearily and confusedly trudges in the company of older systematists who patiently endure the irregularities and inconsistencies of the familiar path. Nothing could show better the conservatism of botanists than the fact that for a third of a century after the general acceptance of the doctrine of evolution they are still using so crude an arrangement of the group of plants with which they are most familiar.' Here and there a voice is raised against the continuance of this archaic system, and now and then one breaks his allegiance to Bentham and Hooker and becomes a disciple of Eichler, but the inconvenience, and the difficulties encountered in inaugurating the

new system in the herbarium, and still more in the class room, have been sufficient practically to maintain the old system in our colleges, universities, herbaria, botanic gardens, and what is, of more importance, our text-books. There are to-day but one or two American text-books, and these quite elementary ones, in which the student can learn aught of any other than the prevailing Benthamian system. In the manuals which the student uses there is no hint as to the relationship of the groups. The beginner soon learns that families which are in juxtaposition are usually more or less related, but he meets with some puzzling cases as when he finds the Labiatæ, Plantaginaceæ and Nyctaginaceæ arranged side by side, with no word of explanation. Even in the more pretentious works, as in the "Synoptical Flora," and in the monographs of families, we have little if any discussion of the question of real relationship, its place being taken by a very little reference to what may be termed "descriptive affinity."

I may assume that it is well known to nearly all of us that the prevailing arrangement of the Dicotyledons does not represent the later views of any of the systematists. Bentham and Hooker distinctly state at the beginning of their treatment of the Apetalæ that the group is one neither natural nor well limited, and suggest in connection with many of the families their relationship to many Polypetalæ. While their arrangement follows the Candollean sequence, their explanatory notes show that the authors recognized the fact that the plants gathered in the group Apetalæ are reduced forms of Polypetalæ and Gamopetalæ.

Even so conservative a writer as Dr. Gray admits[1] that the prevailing system consists of "tenative groupings" of the families, and that "most apetalous flowers are reductions or degradations of polypetalous types." And yet a little later the same writer gives his adhesion to the Candollean system, in spite of the fact that he had declared[2] that viewed in the light of evolution "affinity is consanguinity, and classification in so far as it is natural, expresses real relationship." Here we have a principle of classification worthy of modern science, but a practice which abandons or ignores it. Thus he says[3] "the apetalous and achlamydeous must be the lowest," and describes them as "those plants which, with

Structural Botany (189), pp. 342. 344. [2] *Ibd*, p. 329. [3] *l. c.*

all their known relations, are most degraded or simplified by abortions and suppressions of parts which are represented in the complete flower." Commenting on these he says "These are low in structure, equally whether we regard them as reduced forms of higher types, or as forms which have never attained the full development and diversification which distinguish nobler orders." These remarks are closely followed by the presentation of a synoptical view[1] of the "present received" system.

The fact is, that the systematic disposition of the higher plants is at present a makeshift, maintained by conservatism and a reverence for the time-honored work of the fathers. It is unscientific to let our practice drag behind the present state of our knowledge: it is far more so for us to cling to the opinions of our fathers through mere reverence, long after we know them to be untenable. It is not to the credit of our science that for a second time she has persistently held to a system through such considerations. For thirty or forty years after a natural system had been constructed by Jussieu, botanists, as a body, still adhered to the artificial system of Linné. As late as 1830, Lindley,[2] in urging the abandonment of the Linnean system, refers to the current objections to the natural system in these words: "Its uncertainty and difficulty deter us, say those, who acknowledging the manifest advantages of the natural system, nevertheless continue to make use of the artificial method of Linnæus."

It was not until 1833 that Beck gave us the first American Manual,[2] in which the natural system was adopted. Three years later Dr. Gray brought out his "Elements of Botany,"[3] and so common was the Linnean system that he devotes no less than eighteen pages to its full exposition. He finds it necessary, moreover, to argue at length that the Linnean system, while originally adopted by its founder as a temporary substitute for a natural system, had ceased to be necessary or useful, and to plead with those who would perpetuate it to consider whether the respect due to the system be not compromised "by permitting its degradation to purposes for which it was not originally designed."

Now sixty years later we find ourselves faced with a problem similar to that which Lindley, Torrey, Beck, and Gray met. His-

[1] "Introduction to the Natural System of Botany," p. xii.
[2] "Botany of the Northern and Middle States," by Lewis C. Beck. 1833.
[3] "Elements of Botany," by Asa Gray. New York, 1836.

tory repeats itself with such exactness, that with the change of a word here and there the arguments, pro and con, then used may be used to-day. The system of Jussieu and De Candolle is now as much a clog and a hinderance to the systematic botany of the higher plants, as was that of Linné sixty years ago; and now, as then, it is the spirit of conservatism and of veneration for time-honored usage which maintains the incubus.[1]

Manifestly a system of classification which conforms to and is based upon the doctrine of evolution must begin with those forms which are primitive, or which, as nearly as may be, represent primitive forms. Since the flower is a shoot, in which the phyllomes are modified for reproductive purposes, that flower in which the phyllomes are least modified, must be regarded as primitive, while that in which there is most modification must be regarded as departing most widely from the primitive type. The simple pistil, developed from a single phyllome, is primitive and lower; the compound pistil is derived, and higher. The several-seeded compound ovary must be lower, and the compound ovary, with but one seed, must be higher. Separate stamens are primitive, united stamens, whether the union be with one another, or with other structures, must be derived, and consequently higher. So, too, when all parts of the flower are separate, it is a primitive condition, and when they are united it is a derived structure.

Applying these principles to the flowering plants, it becomes evident that in the Dicotyledons, either the Apetalæ or the Polypetalæ must furnish our starting-point. The Gamopetalæ are universally admitted to be higher than the groups just mentioned, and certainly do not contain the sought-for primitive types. Even a hasty examination of the thirty-six apetalous families, shows that they are at least, to a very large extent, derived from the Polypetalæ by the abortion of some parts and the entire omission of others. We may at once reject all of Bentham and Hooker's Series I, the Curvembryeæ, for we have here a much modified compound pistil, which with other structural peculiarities, shows them to be reduced from the Caryophylline type. Series II (Podostemaceæ), and III (Nepenthaceæ, Cytinaceæ, and Aristolochiaceæ) have compound pistils and must be rejected. In

[1] I have not deemed it necessary to refer to the position accorded to the Gymnosperms in most of our systematic works. It is so manifest an error as to need no discussion here. The flowering plants referred to in this paper are the Angiosperms, both Monocotyledons and Dicotyledons.

Series IV, the Piperaceæ have compound pistils, and the three remaining families, with simple pistils, are with little doubt to be regarded as reduced from the Ranunculine type. Series V (the Daphnales) may be transferred bodily to the Celastrales in the Polypetalæ, of which they are apparently reductions. The parasitic and parasitically-inclined members of Series VI (Loranthaceæ, Santalaceæ, and Balanophoreæ) are out of the question, being too evidently not of primitive type. The great aggregation of plants, brought together under Series VII and VIII, is composed almost entirely of forms with compound, and often much modified, pistils.

It appears, then, that when we search for families in the Apetalæ which may satisfy the requirements of a primitive group, from which the Dicotyledons may have evolved, we find none which will serve our purpose. The dictum of Dr. Gray[1], that "the apetalous and achlamydeous must be lowest," is not sustained. We must accordingly turn to the Polypetalæ, where we find all degrees of complexity from the Ranales, with all the parts of the flowers simple and separate, to the Umbellales with a compound, inferior, much modified gynœcium. It will not be difficult to determine that the Ranales must take rank below all other Polypetalæ, in the sense of representing more nearly than any other group the primitive Dicotyledons. This position was long ago suggested by the younger Jussieu,[2] who commenting on the fact that at that time most authors looking upon the flowers of the Ranunculaceæ and their relatives as more perfect than any other, would place them at the head of the vegetable kingdom,—argued, on the contrary, that the whole structure of the flower in those plants, and especially the easy passage from the organs of vegetation to those of reproduction, required that they must be assigned not only to a lower position, but to one exactly opposite to that which they usually occupied. He significantly indicates that the Compositæ, with a "precisely inverse" structure, should be regarded as the highest of the Dicotyledons.

The Ranales have distinct and separate perianth leaves (sepals and petals); the stamens are distinct, free from either perianth or pistils, and are mostly indefinite and many in number; the pistils are simple, free from the stamens or the perianth, and usually

[1] "Structural Botany," p. 343.
[2] "The Elements of Botany." Translated by Wilson. 1848, p. 543.

many; the ovules are many to few or one, and have two coats; the embryo is small and surrounded by a large endosperm. They satisfy very well the theoretical structure of a primitive dicotyledonous plant, and we may well use them as the point of departure for our system.

De Candolle, and his followers in phanerogamic morphology, took the flower of the Ranales as their typical or pattern flower, and built up a morphological system in which the underlying principle was the theory that all the forms of flowers are due to modifications of this archetype. We are all familiar with this treatment of the structure of the flower, as it is the one so clearly and ably set forth by Dr. Gray in his "Lessons" and "Structural Botany." In accordance with this morphology the Candollean sequence of the families of Dicotyledons was wrought out, the successive families showing greater and greater departures from the archetype. It is remarkable that this sequence, so established, did not more forcibly suggest what is now so plain that every man may read it—the greater principle of a genetic evolution. The old morphologists builded better than they knew, and though they did not understand it, gave us an arrangement of the principal polypetalous plants which accords wonderfully with our present knowledge of the evolution of living things. Yet, they interpreted departure from the type or pattern flower as a lower modification, and at first made a descending series.

We may, then, in the light of evolution accept in general the Candollean plan of arrangement of the Polypetalæ, as wrought out by Bentham and Hooker in their "Genera Plantarum," and adapt it to the new view, with comparatively few changes. The apetalous families may be assigned to places near those to which they are evidently related, and if any remain whose relationship cannot be made out, they may be placed in some such convenient limbo as the "Ordines Anomali" of Bentham and Hooker, or the suggestive "Anhang" of the German botanists.

Our system then begins with the Ranales, to which we have added the small families Myristicaceæ, Monimiacedæ, and Chloranthaceæ. The Parietales, Polygalales, and Caryophyllales follow, the latter much increased by the apetalous families of the so-called Curvembryeæ, and the Salicaceæ from Bentham and Hooker's "Ordines Anomali." The Guttiferales and Malvales close the Thalamifloral series, but the latter group has been greatly increased

by the addition of a number of apetalous families, viz., Piperaceæ, Euphorbiaceæ, Balanopseæ, Empetraceæ, Urticaceæ, Leitneriaceæ.

In the Discifloræ, which originate near the Ranales, we have (1) the Geraniales; (2) the Celastrales (in the wider sense, including Olacales), to which are now added the apetalous Daphnales, and Achlamydosporeæ; and (3) the Sapindales, to which have been added the apetalous Juglandaceæ, Cupuliferæ, and Myricaceæ, and possibly the Casuarineæ.

Going back again to the vicinity of the Ranales, we find the initial point from which the Calycifloral series develops through the Rosales, to the Myrtales, Passiflorales, Cactales, and Umbellales, the highest of the polypetalous plants.

A careful study of the Gamopetalæ, while showing that they rank higher than Polypetalæ, brings out the fact that the group is not a single one, and has probably arisen by development along, at least, two lines. From the Caryophyllales in the Thalamifloræ, the small series Heteromeræ came off, including the Ericales, Primulales, and Ebenales, and these connect with the Bicarpellatæ, composed of the Gentianales, Polemoniales, Personales, and Lamiales. From the summit of the Calcyfloræ we pass easily to the Inferæ, reaching first the Rubiales, which connect equally with the Campanales and Asterales, the last the highest of the Dicotyledons.

A similar examination of Bentham and Hooker's monocotyledonous groups, which they name "series," shows that the sequence which they adopt gives no idea of their probable method of evolution. Here their series Apocarpæ (Alismales), so strikingly like the Ranales of the Dicotyledons, is without doubt the primitive one, and this leads easily to their Coronarieæ (the Liliales of other authors). From this central group two diverse sets of branches have developed, the one with a superior, and the other an inferior compound ovary. In the hypogynous branches there is at once a reduction in the parts of the flower, as is well shown in the Nudifloræ (Arodiales), and Calycinæ (Palmales), culminating in the Glumaceæ (Glumales), which have so far departed from the lily type that at first sight it is difficult to see any relationship. The other branch passes by easy steps through the Epigynæ (Iridales) to the Microspermæ (Orchidales).

Such a system as is here briefly outlined, while it cannot hope to

be free from grave errors, may yet claim for itself that it is an attempt at such an arrangement of the families of flowering plants as would be in harmony with the doctrine of evolution.

May I beg of you that you will not underrate the importance of a general system, and I hardly need to remind you that unless the families of plants are arranged in accordance with some system, our classification is arbitrary and artificial. The attempt to make a natural system by linking family to family in a long undulating chain, by concatenation, is unscientific, because it absolutely fails to conform to the law of evolution. We must abandon the old classification and attempt one, which in the light of evolution, is rational. Let us not cling to the old because it is inconvenient to change—let us not cling to it through a mistaken reverence for the practice of the fathers—let us not cling to it as long as a flaw may be found in a new system. Science is ever abandoning the old, when the old is no longer the true; it tears down the work of years, when that work no longer represents the truth; and it dares to reach out and frame a rational system, even though some parts of it, for a time, rest upon hypothetical grounds.

## A REVISED ARRANGEMENT OF THE BENTHAMIAN "SERIES" OF FLOWERING PLANTS.

### SUB-CLASS MONOCOTYLEDONES.

APOCARPÆ. (Alismales.)
Alismaceæ, Triurideæ, Naiadaceæ.

CORONARIEÆ. (Liliales.)
Roxburghiaceæ, Liliaceæ, Pondeteriaceæ, Philydraceæ, Xyridaceæ, Mayaceæ, Commelinaceæ, Rapateaceæ.

NUDIFLORÆ. (Aroidales.)
Pandanaceæ, Cyclanthaceæ, Typhaceæ, Aroideæ, Lemnaceæ.

CALYCINÆ. (Palmales.)
Flagellarieæ, Juncaceæ, Palmaceæ.

GLUMACEÆ. (Glumales.)
Eriocauleæ, Centrolepideæ, Restiaceæ, Cyperaceæ, Gramineæ.

HYDRALES.
Hydrocharideæ.

EPIGYNÆ. (Iridales.)
Dioscoreaceæ, Taccaceæ, Amaryllidaceæ, Iridaceæ, Hæmodoraceæ, Bromeliaceæ, Scitamineæ.

**MICROSPERMÆ.** (Orchidales.)
Burmanniaceæ, Orchidaceæ.

SUB CLASS DICOTYLEDONES.

*Polypetalæ.* (*Choripetalæ.*)

**THALAMIFLORÆ.** Hypogynous, apocarpous, or syncarpous.
Ranales.
Ranunculaceæ, Dilleniaceæ, Calycanthaceæ, Magnoliaceæ, Anonaceæ, Myristicaceæ, Monimiaceæ, Chloranthaceæ, Menispermaceæ, Berberidaceæ, Nymphaeaceæ.
Parietales.
I. Sarraceniaceæ, Papaveraceæ, Cruciferæ, Capparidaceæ, Resedaceæ, Cistaceæ, Violaceæ, Canellaceæ, Bixaceæ,
II. Nepenthaceæ.
Polygalales.
Pittosporaceæ, Tremandraceæ, Polygalaceæ, Vochysiaceæ.
Caryophyllales.
I. Frankeniaceæ, Caryophyllaceæ, Portulacaceæ, Tamariscaceæ, Ficoideæ.
II. Nyctaginaceæ, Illecebraceæ, Amarantaceæ, Chenopodiaceæ, Phytolaccaceæ, Batideæ, Polygonaceæ.
III. Salicaceæ.
Guttiferales.
Elatineæ, Hypericaceæ, Guttiferæ, Ternstrœmiaceæ, Dipterocarpeæ, Chlænaceæ.
Malvales.
I. Malvaceæ, Sterculiaceæ, Tiliaceæ.
II. Euphorbiaceæ, Balanopseæ, Empetraceæ.
III. Urticaceæ, Platanaceæ, Leitneriaceæ, Ceratophyllaceæ.
IV. Piperaceæ (?), Podostemaceæ.

**DISCIFLORÆ.** Hypogynous, syncarpous.
Geraniales.
Linaceæ, Humiriaceæ, Malpighiaceæ, Zygophyllaceæ, Geraniaceæ, Rutaceæ, Simarubaceæ, Ochnaceæ, Burseraceæ, Meliaceæ, Chailletiaceæ.
Celastrales.
I. Olacaceæ, Ilicineæ, Celastraceæ, Stackhousieæ, Rhamnaceæ, Ampelideæ.
II. Lauraceæ, Proteaceæ, Thymelæaceæ, Penæaceæ, Elæagnaceæ.
III. Santalaceæ, Loranthaceæ, Balanophoraceæ.
Sapindales.
I. Sapindaceæ, Sabiaceæ, Anacardiaceæ.
II. Juglandaceæ, Myricaceæ, Cupuliferæ.

III. ? Casuarinaceæ.

CALYIFLORÆ. Epigynous, syncarpous.

Rosales.

Connaraceæ, Leguminosæ, Rosaceæ, Saxifragaceæ, Crassulaceæ, Droseraceæ, Hamamelidaceæ, Bruniaceæ, Halorageæ.

Myrtales.

I. Rhizophoreæ, Combretaceæ, Myrtaceæ, Melastomaceæ, Lythraceæ, Onagraceæ.

II. ?Aristolochiaceæ, Cytinaceæ.

Passiflorales.

Samydaceæ, Loasaceæ, Turneraceæ, Passifloraceæ, Cucurbitaceæ, Begoniaceæ, Datiscaceæ.

Cactales.

Cactaceæ.

Umbellales.

Umbelliferæ, Araliaceæ, Cornaceæ.

*Gamopetalæ.*

HETEROMERÆ. Ovary, mostly superior.

Primulales.

Plumbaginaceæ, Plantaginaceæ, Primulaceæ, Myrsinaceæ.

Ericales.

Vacciniaceæ, Ericaceæ, Monotropeæ, Epacrideæ, Diapensiaceæ, Lennoaceæ.

Ebenales.

Sapotaceæ, Ebenaceæ, Styracaceæ.

BICARPELLATÆ. Ovary mostly superior; stamens and petals isomerous; ovary, bi-carpellary.

Gentianales.

Oleaceæ, Salvadoraceæ, Apocynaceæ, Asclepiadaceæ, Loganiaceæ, Gentianaceæ.

Polemoniales.

Polemoniaceæ, Hydrophyllaceæ, Boraginaceæ, Convolvulaceæ, Solanaceæ.

Personales.

Scrophulariaceæ. Orobanchaceæ, Lentibulariaceæ, Columelliaceæ, Gesneraceæ, Bignoniaceæ, Pedaliaceæ, Acanthaceæ.

Lamiales.

Myoporineæ, Selagineæ, Verbenaceæ, Labiatæ.

INFERÆ. Ovary inferior, stamens and petals isomerous.

Rubiales.

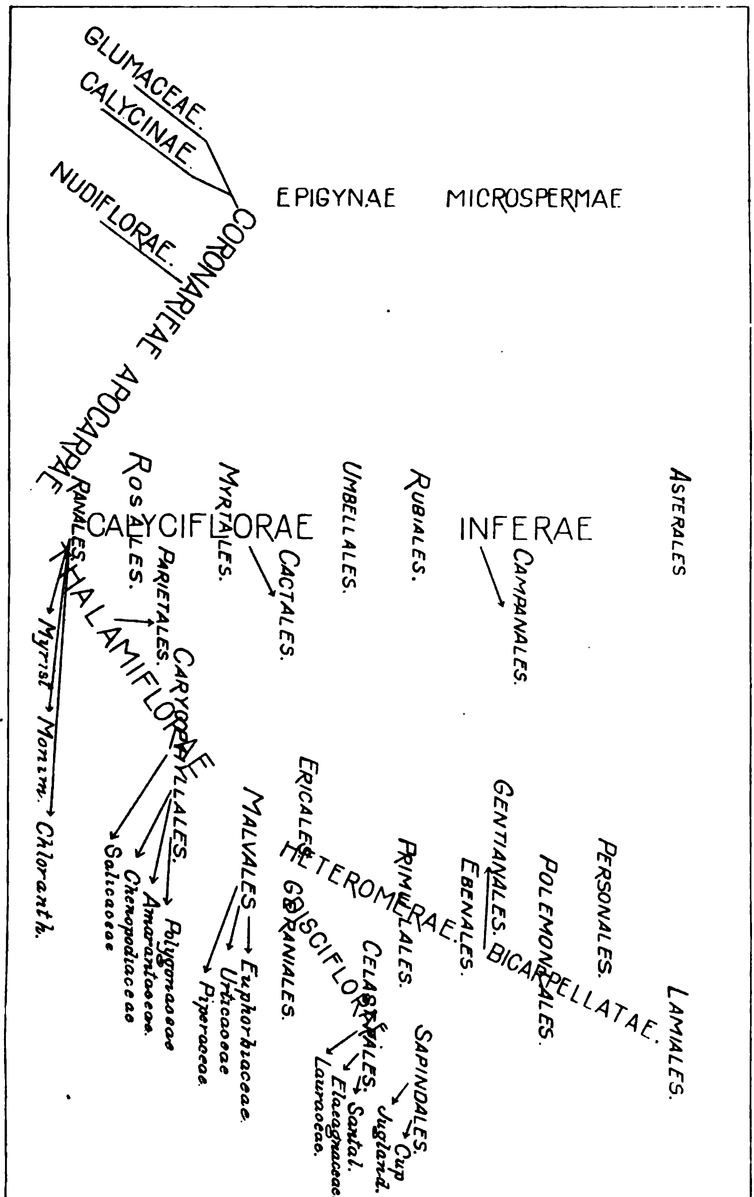
Caprifoliaceæ, Rubiaceæ.

Campanales.

Stylidaceæ, Goodenovieæ, Campanulaceæ.

Asterales.

Valerianaceæ, Dipsaceæ, Calyceraceæ, Compositæ.




DIAGRAMMATIC REPRESENTATION OF THE RELATIONSHIP OF THE SERIES OF FLOWERING PLANTS.

# PAPERS READ.

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COMPARATIVE STUDY OF THE STRUCTURE AND FUNCTION OF THE SPORANGIA OF FERNS IN THE DISPERSION OF SPORES. By GEO. F. ATKINSON, Ithaca, N. Y.

[ABSTRACT.]

IN examining the sporangia of the different families of ferns to determine the character of the so-called complete annulus, especially in the Cyatheaceæ and Gleicheniaceæ, as distinguished from the incomplete annulus of the Polypodiaceæ, some points of interest were observed in connection with the function of different parts of the sporangia in the dispersion of spores, and the extent of the true annulus.

In the Polypodiaceæ the annulus extends from the lower posterior angle of the sporangium (directly in line with the stalk) over to the upper anterior angle. The lip-cells in the middle of the front determine the line of cleavage at the moment of dehiscence. The other cells of the front form two connectives; the upper one connects the upper lip-cell with the anterior end of the annulus, while the lower one connects the lower lip-cell with the stalk.

In *Cyathea brunonis* and *Cibotium chamissoi* of the Cyatheaceæ strictly homologous divisions are found in the so-called complete annulus. The true annulus, *i. e.*, the cells which serve as the spring, extends from the lower anterior angle of the sporangium backwards past the side of the stalk, up the posterior side to the upper anterior angle. A series of four to six lip-cells similar in appearance to those of the true annulus, but smaller, occupies the middle of the front. Between two of these the line of cleavage occurs. An upper and lower connective, each consisting of two or three cells exactly like those of the lateral walls, interrupts the so-called "complete" annulus.

In *Hymenophyllum ciliatum* and *H. demissum* of the Hymenophyllaceæ the true annulus is nearly complete. The very short stalk is attached almost perpendicularly to the lateral wall of the sporangium by the side of one end of the annulus. Narrow elongated lip-cells are present, joined to the annulus by two small connectives.

In *Gleichenia emarginata* of the *Gleicheniaceæ* the middle of the true annulus passes near the point of attachment of the sessile, obclavate sporangium. Upon the other side are rows of elongated lip-cells determining the line of cleavage. These are separated from the ends of the true

annulus by two connectives each composed of two or three cells exactly like those of the walls of the sporangium.

In *Osmunda* and *Todea* of the Osmundaceæ the connectives are very large, while in *Schizæa* and *Aneimia* of the Schizæaceæ they are quite small, since the true annulus nearly encircles the narrow apex of the ovate sporangium.

In dehiscence the relation of the lip-cells to the true annulus, divides the sporangium into two nearly, or quite equal parts. The connectives prevent the lateral walls from being torn badly, so that they hold the spores until the annulus springs. Were the annulus, that is that part which serves as the spring, complete, the entire sporangium would be everted and the spores dropped before the annulus springs, and the effective dispersion of the spores would be prevented.

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SYMBIOSIS IN THE ROOTS OF THE OPHIOGLOSSEAE. By GEO. F. ATKINSON, Ithaca, N. Y.

[ABSTRACT.]

WHILE studying the structure of the root of *Botrychium virginianum* yellowish protoplasmic masses in certain cells of the cortical parenchyma, producing a strong contrast with the starch content of the other cells of the cortex, attracted my attention. Close examination and treatment with reagents showed the existence, in the cortical parenchyma of the root, of a symbiotic fungus stimulating the cells to the development of a rich protoplasmic content.

The organism is located at quite a definite portion of the cortical parenchyma, about one-third to one-half the distance from the epidermis to the central cylinder. In transection of the root, it appears in the form of a circle. From this localized center numerous threads extend to the epidermis and the outside of the root. The threads branch profusely and at the seat of metabolic activity as a symbiont they are partly immersed in the masses of protoplasm.

Since the roots of the Ophioglosseæ do not possess root hairs, or only a slight development of them, it occurred to me that possibly the presence of this symbiotic organism was universal in the roots of plants belonging to this order. Accordingly, several species both of Botrychium and Ophioglossum, most of them in the Horace Mann herbarium of Cornell University, were examined. In all of them the presence of the organism was determined.

The species examined together with the localities are here appended: *Botrychium matricariæfolium*, *B. ternatum* and *B. virginianum* from New York; *B. lanceolatum* from New Hampshire; *B. lunarioides*, Massachusetts and Vermont; *B. subbifoliatum*, Hawaiian Islands; *B. lunaria*, Cloon Mountains; Botrychium (No. 484) Drummond from Louisiana; *Ophioglossum vulgatum*, New York and the suburbs of Paris; *O. lusitanicum*, Sardinia and the Island of Madeira; *O. palmatum*, eastern Cuba; *O. pendulum*,

Oahu, Hawaiian Islands. A note accompanies the latter to the effect that it was collected on trees, which is quite strong evidence of the probable necessity for the presence of this symbiont.

It thus appears that in the Ophioglosseæ throughout the world, there exists a close symbiotic relationship with this organism, in all probability an accompaniment, or the cause of the absence of root hairs, which may have disappeared through the lack of the necessity for such absorbent organs.

No specimens of Helminthostachys were at hand to examine, but I think we may confidently expect to find the organism in this genus of the Ophioglosseæ also.

It may still be a question, how much influence this symbiont has had on the simplicity of structure found in the Ophioglosseæ, and what effect then this would have on the phylogenetic position of the order.

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PHOTOGRAPHY AS AN INSTRUMENT FOR RECORDING THE MACROSCOPIC CHARACTERS OF MICROORGANISMS IN ARTIFICIAL CULTURES. By GEO. T. ATKINSON, Ithaca, N. Y.

[ABSTRACT.]

MANY species of microorganisms in artificial nutrient media present, in the growth of the colonies, characteristic peculiarities of form. These macroscopic appearances are frequently of great value as differential characters. Some species, especially of fungi, when viewed by transmitted light, present important characters in the fine radiating threads and the general arrangement of the colony as a whole. The comparative density also of the colony is frequently quite constant. A method of accurately recording these macroscopic characters would be a valuable aid in description and comparative study.

Where the growth is colorless, not very dense, and peculiar for the fineness of its meshes or radiations, it would be difficult to photograph the colonies by ordinary methods of exposure, since there is little difference in color between the medium and the object. A sensitive plate in an ordinary camera exposed to a plate culture by perpendicular rays of transmitted light shows little differentiation between the medium and colonies after development The differentiation is also weak on the ground glass.

When, however, the perpendicular rays are cut off and oblique rays from several different directions are thrown through the plate culture upon the sensitive plate, the colonies are differentiated strongly in all their exquisite forms and tracings. The culture plates or tubes are inserted in an opening in the end of a box. The lens of the camera is pointed toward a window and the box with the culture is placed between the window and lens in order to obtain transmitted light through the culture. A perfectly black screen, 30 to 40 cm. in diameter, is then to be hung upon the window directly in front of the object in order to cut off the perpendicular rays of light.

ON THE FOOD OF GREEN PLANTS. By Prof. CHARLES R. BARNES, Madison, Wis.

[ABSTRACT.]

THE constant tendency of biological science is to minimize the differences between the physiological processes of plants and animals. Having a common starting point in its physical basis it is natural to expect that the manifestation of life in plants and animals will be essentially similar. Although this fundamental similarity has been generally recognized, yet in details it has been frequently overlooked. Particularly is this the case with text books, both elementary and advanced.

Among the supposed differences between green plants and animals none has been more persistently urged than this: Green plants live chiefly upon inorganic food obtained in the form of $CO_2$, $HO^2$, and mineral salts, whereas animals require organic food. It is pointed out in this paper, first: That in a scientific sense the terms organic and inorganic are nearly or quite obsolete among chemists. Most organic substances belong to a group more correctly known as carbon compounds, whose connection is very intimate. Among these compounds, the most stable one, anhydrous carbon dioxide, $CO^2$, naturally finds a place. While it is not possible to use the terms organic and inorganic with scientific accuracy, because they are not scientific, if we endeavor to use them as correctly as our present knowledge demands, we cannot say that the food of green plants is inorganic, except in so far as the mineral salts and water are concerned.

But the author undertakes to show secondly: That carbonic acid can not properly be considered as the *food* of green plants. It has been almost demonstrated that the first product of the alteration of carbonic acid is formic aldehyde, and that this substance is built up, probably by polymerization, into complex carbohydrates.

Third: That it is highly improbable, both from the chemical and physiological side, that the protoplasm is able to use carbonic acid directly, and to bring molecules of C, H, and O, into such relations to the proteid molecules as to form any part of them. Therefore carbonic acid is not to be considered as a food.

Fourth: It is known that carbohydrates disappear during growth and repair. These substances, therefore, are to be looked upon as the food of the protoplasm.

This process of the manufacture of carbohydrates has been called "assimilation," "assimilation proper," or the "assimilation of carbon." None of these terms is appropriate, since we ought not to use a term of long standing in animal physiology in a totally different sense for vegetable physiology, and the term "photosyntax" is suggested to replace the phrases quoted.

It is also shown that the alterative processes to which solid foods are subject in the plant body may properly be termed digestion.

The paper closes with the following definitions: *Photosyntax* is the synthesis of complex carbon compounds out of carbonic acid. *Digestion*

consists in the chemical change and solution of solid foods, due in large measure, perhaps entirely, to the action of alterative enzymes. *Assimilation* is the conversion of these compounds into the living or mechanical substance of the plant tissues for repair of waste and growth. *Food* is the physiological term for all substances capable of direct assimilation or of digestion and assimilation.

[This paper has been printed in the *Botanical Gazette*.]

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THE MINUTE STRUCTURE AND DEVELOPMENT OF THE MOTILE ORGAN IN THE LEAF OF THE RED BUD. By S. G. WRIGHT, Lafayette, Ind.

[ABSTRACT.]

LEAF movement has been observed since the time of Pliny, but Hooke in 1667 was the first to undertake an explanation of the causes of the phenomena. Dutrochet, in 1822, was the first to introduce the correct ideas of plant movement.

The red bud, on account of its active leaf movement, and the size of its motile organs, is a very desirable plant for dynamical and histological work, and a brief study of its pulvini leads to the following conclusions:

By observations made at different times during the day the lamina of the leaf was found to move through an angle of approximately 100°. The pulvinus, situated at the base of the lamina, is found to consist mostly of loose irregular parenchyma.

The first differentiation of this irregular tissue takes place in the miniature leaves of the unopened bud, and so far as investigated this development of irregular cells is in no wise essentially different from that taking place in the mesophyll tissue of the lamina of the leaf.

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THE SHRINKAGE OF LEAVES IN DRYING. By Prof. BYRON D. HALSTED, New Brunswick, N. J.

[ABSTRACT.]

IT is well known that foliage in drying undergoes considerable shrinkage. By means of the Solandi process this shrinkage can be studied fully and accurately. Impressions of green leaves are first taken, and if desired they may be obtained each day until the foliage is fully dried.

By means of many series of impressions it is shown that the amount of shrinkage not only varies with different kinds of leaves, but the loss in size depends much upon the age of the leaf. Young leaves of full size shrink more than older ones.

There is a marked difference between the shrinkage of wilted and parallel veined leaves. The latter, as a rule, only shrink in width, as well shown in the lily-of-the-valley, orchid and the grass. Some examples of

surprising shrinkage were shown in leaves of catalpa, plantain and other plants where the decrease in size was more than half, by the ordinary process of herbarium press drying.

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THE SOLANDI PRINTING APPLIED TO BOTANICAL WORK. By Prof. BYRON D. HALSTED, New Brunswick, N. J.

[ABSTRACT.]

THIS process consists in making impressions by transmitted sunlight upon sensitized paper (Aristo preferred) and after toning, this impression becomes the negative from which a positive is made in the same manner as the negative was produced in the printing-frame in the sunlight.

The negative is treated with kerosene to make it more translucent. Dry subjects, as leaves and sections of wood, etc., are treated in the same manner for the production of the negative.

A large number of illustrations of the work are shown, consisting of the subject, negative and positive, each mounted in a series upon stiff paper.

The process is rapid, easy and inexpensive, and will give results with leaves and like subjects, superior to those by ordinary photography, for it produces an interior instead of simply an exterior view.

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TWO NEW AND DESTRUCTIVE DISEASES OF CUCURBITS: (1) THE MUSK-MELON ALTERNARIA; (2) A BACTERIAL DISEASE OF CUCUMBERS, CANTALOUPES AND SQUASHES. By ERWIN F. SMITH, U. S. Department of Agriculture, Washington, D. C.

[ABSTRACT.]

I. SYNOPSIS OF A PAPER ON THE MELON ALTERNARIA.

(1) A destructive muskmelon disease not previously known in this country or abroad. Discovered in southwest Michigan in the fall of 1892.

(2) Disease associated with an Alternaria.

(3) Histological examination showed the constant presence of the mycelium of this fungus in the diseased spots and the absence of other fungi, and made it almost certain that the Alternaria was the cause of the disease.

(4) Details of so much of the life history of the fungus as could be learned from artificial cultures on agar.

(5) Account of infection experiments which showed the Alternaria to be as distinctly parasitic as any Peronospora and put the cause of the disease beyond dispute.

(6) Account of an Italian melon disease which appeared in the summer of 1892 and was studied by Dr. Peglion of Avellino.

(7) Cultures, histological examinations, and successful infection experiments with material received from Italy make it certain, or almost certain, that these two destructive outbreaks at the same time and in widely different parts of the world were due to the same fungus.

(8) Notes on various fungi which grew in the cultures and two of which are proved to be genetically related and are suspected of belonging to the life cycle of Alternaria, but the proof of which is insufficient.

### II. SYNOPSIS OF PAPER ON A BACTERIAL DISEASE OF CUCUMBERS, CANTALOUPES AND SQUASHES.

(1) Account of a destructive cucumber, melon and squash disease which appeared in the dry summer of 1892 in the vicinity of Washington, D. C.

(2) Disease not due to fungi or insects.

(3) Histological studies in connection with the symptoms put the bacterial origin of the disease almost beyond doubt.

(4) Infections with pure cultures not yet secured.

(5) The disease begins in the blade of the leaf, the bacillus making its way into the stem through the spinal vessels.

(6) The destructive action of the organism is confined chiefly to the xylem part of the bundle the spirals first filling with the bacilli and then the larger netted and pitted vessels.

(7) The sudden general wilt of the plant is due to the filling of the vessels and the stopping of the water current

(8) Cavities are formed in the tissue surrounding the spiral vessels, and are full of bacilli.

(9) Externally the stems appear uninjured.

(10) The disease is almost certainly transmitted by insects.

[To be printed by U. S. Dept. of Agric.]

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**DEVIATION IN DEVELOPMENT DUE TO THE USE OF UNRIPE SEED.** By Prof. J. C. ARTHUR, Lafayette, Ind.

[ABSTRACT.]

ATTENTION is called to the circumstance that immature seeds will grow into apparently perfect plants, a fact that has been known for a long time. The relation of viability and maturity is discussed. The optimum maturity for germination is pointed out, and the fact that the percentage of germinations will be less, the greater the distance from this point, either toward immaturity or over-maturity. The chief deviations from normal development due to use of immature seeds are stated to be (1) weakening of the seedling, shown in the fewer number of germinations, slower rate of growth, and larger proportion which succumb to bad weather, as well as the failure of the plants to attain full size and strength

(2) preponderance in development of the reproductive over the vegetative portion, and finally (3) earlier maturity. By way of explanation, these phenomena are considered as belonging to a special instance under a general law controlling plants (and animals), when the normal uniform growth of the organism has been impeded, or suddenly checked.

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A SCLEROTIUM DISEASE OF PLANTS. By P. H. ROLFS, Lake City, Fla.

[ABSTRACT.]

THIS paper describes a Sclerotium which produces a disease of at least sixteen different plants in five orders, the appearance of the disease varying widely on different plants, even those in the same order.

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AN ANALYSIS OF THE CONDITIONS AFFECTING THE DISTRIBUTION OF PLANTS. By FREDERICK V. COVILLE. Department of Agriculture, Washington, D. C.

[ABSTRACT.]

THE ultimate factors in the distribution of plants are reduced to heat, light, air, food, water, and mechanique. These simple elements are combined to form such compound conditions as soil, climate, etc.

[This paper has been printed in Contributions from the U. S. National Herbarium, Vol. 4.]

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THE PRINCIPAL DISEASES OF CITROUS FRUITS NOW BEING STUDIED AT EUSTIS, FLORIDA. By W. T. SWINGLE, Subtropical Laboratory, Eustis, Fla.

[ABSTRACT.]

THE paper gives short accounts of the principal diseases of citrous fruits not caused by insects, viz., blight, mal di gomma, die-back and scab, together with accounts of methods pursued in the study of such obscure diseases.

[This paper will be printed in Bull. U. S. Dept. Agric. Div. Veg. Path.]

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CEPHALUROS MYCOIDEA AND PHYLLOSIPHON, TWO SPECIES OF SPARASITIC ALGÆ NEW TO NORTH AMERICA. By W. T. SWINGLE, Subtropical Laboratory, Eustis, Fla.

[ABSTRACT.]

NOTES the discovery of *Cephaluros* on leaves of *Magnolia* and *Xanthoxylon* in central Florida. The occurrence of *Phyllosiphon sp.* on leaves of *Arisæmia* is recorded from the Dells of the Wisconsin River.

A REVISION OF THE GENUS PHYSCOMITRIUM. By ELIZABETH G. BRITTON, Columbia College, N. Y.

[ABSTRACT.]

THE types of the North American species of the genus have been examined by the author. Drawings and specimens were shown with descriptions of new species.

[This paper will be printed in Bulletin of the Torrey Botanical Club for May, 1894.]

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DISTRIBUTION OF THE GRAMINEÆ IN THE UNITED STATES. By S. M. TRACY, Agricultural College, Miss.

[ABSTRACT.].

THE paper gives the geographical range of the leading genera of grasses, with notes as to the individual species which are most widely distributed, and is illustrated by diagrams.

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FURTHER OBSERVATIONS ON THE FERMENTATION TUBE WITH SPECIAL REFERENCE TO ANAËROBIOSIS, REDUCTION AND GAS PRODUCTION. By Dr. THEOBALD SMITH, Dept. Agriculture, Bureau of Animal Industry, Washington, D. C.

[ABSTRACT.]

THE fermentation tube is specially adapted to demonstrate the following physiological characters of bacteria:

Aërobiosis and anaërobiosis; fermentation of carbohydrates with the formation of gases and acid products, and the reducing or deoxidizing action of many species. The tube is particularly helpful in determining minor differential characters as expressed by the capacity to ferment one or more carbohydrates.

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THE FRUCTIFICATION OF JUNIPERUS. By JOHN G. JACK, Jamaica Plain, Mass.

[ABSTRACT.]

THIS paper refers to the length of time required by different species of Juniperus to mature their fruit. Various authors have assumed that they were annual-fruited, others have stated that they were biennial, and some have divided the genus into species fruiting annually and those fruiting biennially.

Investigation shows that, of the three species found east of the Mississippi River, the red cedar or savin, *Juniperus virginiana*, is annual-fruited;

*Juniperus sabina procumbens* is biennial; while the common juniper, *Juniperus communis*, does not ripen fruit until the autumn of the third year after flowering.

[This paper was published in the Botanical Gazette, Vol. XVIII (Oct. 1893), plate XXXIII.]

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NOTES ON THE DEVELOPMENT OF MARATTIA DOUGLASII. By Prof. DOUGLAS H. CAMPBELL, Leland Stanford Jr. Univ., Palo Alto, California.

[ABSTRACT.]

1. MATERIAL upon which the observations were made was obtained in the Hawaiian Islands.
2. Distribution of the Marattiaceæ, geographically and geologically.
3. Résumé of work already published on the Marattiaceæ.
4. Result of the author's investigations upon the prothallium and embryo of *M. douglasii*.
5. Conclusions concerning the affinities of the Marattiaceæ with other ferns, and also with the Anthoceroteæ.

[This paper will be printed in Annals of Botany.]

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OBSERVATIONS ON A RUST AFFECTING THE LEAVES OF THE JERSEY OR SCRUB PINE. By B. T. GALLOWAY, U. S. Department of Agriculture, Washington, D. C.

[ABSTRACT.]

INVESTIGATIONS carried on for the past three years are reviewed. The rust is due to a Coleosporium, the life history of which is fully described. The anatomy and physiology of the normal leaf are discussed, after which the changes which lead to the fall of the diseased needle are described.

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RESULTS OF SOME RECENT WORK ON RUST OF WHEAT. By B. T. GALLOWAY, U. S. Department of Agriculture, Washington, D. C.

[ABSTACT.]

THE paper reviews work by the author in 1891-92 on rust of wheat; then describes, somewhat in detail, the results of experiments made in 1892-93 in preventing the disease.

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PROPHYLLA OF GRAMINEÆ. By Prof. WM. J. BEAL, Agricultural College, Mich.

DEFINES prophyllum and compares with the paleæ. Gives results of a study of many species in fifty-six genera. Notes that prophylla in some

genera appear to be quite uniform in shape, while in others they are unlike, and in some variable; notes the relative length of prophylla to the subtending leaf sheath and the difference in the shape of the tips.

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NOTES ON ROESTELIA PYRATA. By Prof. L. H. PAMMEL, Ames, Iowa.

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ON THE QUANTITATIVE ANALYSIS OF THE COLORS OF FLOWERS AND FOLIAGE. By Prof. J. H. PILLSBURY, Smith College, Northampton. Mass.

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THE ROOTS OF ORCHIDS. By M. B. THOMAS, Crawfordsville, Ind. [Printed in the Botanical Gazette.]

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LICHENS OF THE BLACK HILLS AND THEIR DISTRIBUTION. By T. A. WILLIAMS, Brookings, S. D.

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PRELIMINARY STATEMENT CONCERNING BOTANICAL LABORATORIES AND INSTRUCTION IN AMERICAN UNIVERSITIES AND COLLEGES. By Prof. CONWAY MACMILLAN, University of Minn., Minneapolis, Minn.

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A NEW INFECTION-NEEDLE FOR THE STUDY OF LOWER PLANTS. By J. CHRISTIAN BAY, Assistant Missouri Botanical Garden, St. Louis, Mo. [Printed in full in the Botanica. Gazette.]

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THE BIBLIOGRAPHY OF AMERICAN BOTANICAL LITERATURE. By J. CHRISTIAN BAY, Assistant Missouri Botanical Garden, St. Louis, Mo.

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PRESENT ASPECTS OF THE NOMENCLATURE QUESTION. By Dr. N. L. BRITTON, Columbia College, New York, N. Y.

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A CONSIDERATION OF A SPECIES BASED ON THE THEORY OF EVOLUTION. By Dr. N. L. BRITTON, Columbia College, New York, N. Y.

ULOTA AMERICANA, MITTEN AND ORTHOTRICHUM AMERICANUM, BEAUV. By ELIZABETH G. BRITTON, Columbia College, New York, N.Y. [Printed in Bulletin Torrey Botanical Club.]

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PRELIMINARY NOTES ON SOME CHROMOGENIC BACTERIA OF THE AMES FLORA. By Prof L. H. PAMMEL, Ames, Iowa.

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A CASE OF POISONING BY THE WILD PARSNIP, CICUTA MACULATA. By Prof. L. H. PAMMEL, Ames, Iowa.

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CROSSING OF THE CUCURBITS. By Prof. L. H. PAMMEL, Ames, Iowa.

## SECTION H.

# ANTHROPOLOGY.

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# ADDRESS

BY

## JAMES OWEN DORSEY,

VICE-PRESIDENT, SECTION H.

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### *THE BILOXI INDIANS OF LOUISIANA.*

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LADIES AND GENTLEMEN, — In 1885, when I had the honor to be elected chairman of this Section in the absence of Professor Dall, the Vice-Presidential address to which we then listened was entitled "The Native Tribes of Alaska."

I ask you, to-day, to let your thoughts travel in another direction, down from the region of the Yukon to that of the Red River of Louisiana. In the latter country have been found the survivors of a people about whom very little has been written. The very name "Biloxi," by which they are usually called, is unfamiliar to most of us: it has not gained the unpleasant notoriety of Sioux, Apache, and Comanche.

The Indians known to us as the Biloxi call themselves Tanĕks a$^n$ya (variants, a$^n$yadi and ha$^n$yadi). The first word, Tanĕks, is apparently related to tâni, *to be in advance of another*, and, ta$^n$niki, *first*. The second word, a$^n$yadi, means *people*. The whole name, therefore, may be translated, First People. This reminds us of the name by which the Winnebago Indians call themselves, Hotcañgara, *First Speech*, in which tcañga is a variant of a word signifying *first*. A$^n$yadi, *people*, is suggested as the word which was recorded in 1699 as Anani (French, Histor. Coll., II. 99. 1875). A document of 1699, found in Margry (Découvertes, IV. 172), speaks of the Annocchy, whom the Bayougoula called Bilocchy. The appellation Biloxi was first given to the tribe by Pénicaut in 1699 (French, Histor. Coll., New Ser., I. 38. 1869), the first English writer who used this name being Jefferys, in 1761.

Mr. Gatschet gives the following explanation of the origin of the

name Biloxi: "As to the signification of the name it may be stated that in the Cha'hta Nation, Indian Territory, there is, twenty miles south of Eufaula, a little stream called Buk-luktchi, or Creek-terrapin, as it is translated, and that formerly the Indians fished for terrapins there." Mr. Mooney objects to this derivation of the popular name for the tribe, because, as he thinks, Biloxi is very probably a corruption of Taněks or Taněksa, part of their name for themselves; and this agrees with the laws of Siouan consonant changes (t to p, and n to d and l).

The Biloxi were supposed to belong to the Muskhogean stock until, in 1886, Mr. A. S. Gatschet of the Bureau of Ethnology visited the survivors of that tribe, in central Louisiana. To his surprise, he found that many of their words bore strong resemblances to the corresponding ones in languages of the Siouan tribes. He submitted his vocabulary to me, and I soon ascertained that he had made an important discovery. Since then the Biloxi have been known as a people of the Siouan linguistic family.

*Habitats.* — In 1669, according to Drake, the Biloxi had their village on Biloxi Bay, Mississippi, near the Gulf of Mexico. But Margry says that they were on Pascoboula River in 1699. In that year the French settled on Biloxi Bay, on which were three villages, those of the Biloxi, Pascoboula, and Moctoby. This last tribe cannot be identified, but the Pascagoula were associated with the Biloxi in central Louisiana as late as the close of the civil war. In 1805, Sibley spoke of the Biloxi as having come with some French about the year 1763, settling first in Avoyelles Parish, Louisiana. They subsequently removed to Bayou Rapides, near Alexandria, Louisiana, about forty miles below Natchitoches, where they were reduced to about thirty souls in 1805. In 1828 (according to the Soc. Mex. Geogr., 1870), there were twenty families of the Biloxi on the east bank of the Neches River, Texas. It is probable that some of the Biloxi are still in Texas, for, at the close of the late civil war, a party of one or two hundred Pascagoula Indians and mixed-blood Biloxi removed from central Louisiana into Texas, to a place which my Indian informant called "Com'-mish-y." This is probably Commerce, Hunt County, Texas, a railroad town not more than 140 miles northwest of Shreveport, rather than Comanche, which is too far west. According to Gatschet, there are a few Biloxi among the Choctaw and Caddo, besides those on the Apache, Kiowa, and Comanche reservation, Oklahoma; but he has visited only those who were in Avoyelles Parish, Louisiana, in

1886. In 1892, I found a few of the tribe near Lecompte, Rapides Parish, Louisiana, and among them was one of Gatschet's informants. After I had made inquiries of persons in several parts of the State, it was learned that there were no more Biloxi in Avoyelles Parish, the only known survivors of the tribe in the State being near Lecompte and numbering not over a dozen souls. It was during my visits to this people in January and February, 1892, and February, 1893, that the material was collected from which the present address has been prepared. This material consists of thirty texts in the original, with interlinear and free translations and critical notes; many grammatical notes, requiring elaboration; about five thousand entries of words for a Biloxi-English dictionary; several thousand entries for an English-Biloxi dictionary; descriptions of the flora and fauna known to the Indians; and other material, in all sufficient to fill a quarto volume of several hundred pages.

*Former Condition.* — Prior to their coming in contact with the white race, the Biloxi must have been dressed as follows, judging from the recorded names of articles of attire: a man wore the breechcloth, a belt, leggings, moccasins, and garters; around the body was wrapped a robe of bear skin, buffalo skin, deer skin, rabbit skins, or wild-cat skins. In modern times a blanket superseded the skin robe. The women used to wear a loose sacque and a long skirt. No names of other articles of female dress have been obtained. In former days feather head-dresses were worn, as well as necklaces of bone and those made of the white bills of a long-legged red bird (of a species which has not been identified). Nose-rings and ear-rings were common. Within the present century traders have introduced among the Biloxi blankets, necklaces of beads or of silver, handkerchiefs, and tunics of white cotton for men. Betsy, my aged informant, said that her grandmother had tattoo marks on her cheeks, but none on her forehead. The dwellings of this people resembled those found among the northern tribes of the same family: one kind was the low tent of the Osage and Winnebago, the other being the high tent of the Dakota, Omaha, etc. The tribe had wooden bowls, spoons of cow horn or buffalo horn, horn scrapers and fleshers for dressing hides. No names of stone implements have been recorded. The people were skilled in basket-making, and this art is still practiced by old Betsy, who was my chief informant. They used to make pottery, but I could not obtain any names of special implements formed from this material, hence we may infer that the art has been lost.

Their aboriginal linear measure is thus described: From the tip of the nose along the extended arm to the joined ends of the middle and index fingers, which hold one end of the measured article. The thumb was not used in taking this measurement.

*Present Condition.* — The tribal organization has disappeared. Most of the survivors dwell in the pine hills, about seven miles from the town of Lecompte; but there are three persons about half a mile from the town, on the plantation of Mr. J. R. Moore. These three are Betsy Joe, who is about seventy-four years old; her daughter, Maria, who is about twenty-five; and Maria's husband, Bankston Johnson, who is thirty-two years of age. The last is a son of a Biloxi father by an Alabama mother, hence he is reckoned as an Alabama, though his father belonged to the Deer clan of the Biloxi, of which Betsy and her daughter are members. The three survivors just named, as well as the others, have worn civilized attire for years. Johnson and his family, who are the only ones that retain a knowledge of the Biloxi language, are highly respected by their white neighbors.

*Social Organization.* — Descent among the Biloxi was in the female line; hence the social unit is called a *clan*, instead of a gens, as among the Dakota, Ȼegiha, and other Siouan people having descent in the male line. The names of three Biloxi clans have been preserved, though there may have been other clans which have disappeared. 1. The Ita a$^n$yadi, Deer people. 2. The O$^n$ɟi a$^n$yadi, Grizzly Bear People. 3. The Naqoɟod'a a$^n$yadi, Alligator People. Most of the survivors belong to the Deer clan.

*Kinship System.* — The names of fifty-three kinship groups are still remembered, but there are a dozen or more whose name have been forgotten. These fifty-three terms reveal a kinship system far more elaborate than those of the other tribes of the Siouan family. Thus, where the Ȼegiha has but one term, *grandchild*, for all descendants of sons, daughters, sisters' and brothers' sons and daughters, the Biloxi has at least fourteen. In the ascending series, the Dakota and Ȼegiha do not have any terms beyond *grandfather* and *grandmother*. But for each sex the Biloxi has terms for at least three degrees beyond grandfather and grandmother. The Ȼegiha has but one term for father's sister and one for mother's brother, father's brother being called "father," and mother's sister "mother." But the Biloxi has distinct terms for father's elder sister, father's younger sister, father's elder brother, father's younger brother, and so on for the mother's elder and younger brothers and sisters. The Biloxi distinguishes be-

tween an elder sister's son and the son of a younger sister, so between an elder sister's daughter and the daughter of a younger sister. *Father* in Biloxi is adiyaⁿ (root, adi); in Çegiha, iȼadi; in Dakota, ate. *Mother* in Biloxi is oⁿni or uⁿni; in Çegiha, ihaⁿ; in Dakota, ina. *Son* in Biloxi is yiñqi or yiñqiyaⁿ; in Çegiha, ijiñge; in ꓕɔiwere, iyiñe; in Dakota, ciⁿca. *Daughter* in Biloxi is yŭñqi or yŭñqiyaⁿ; in Çegiha, ijañge; in ꓕɔiwere, iyŭñe; in Dakota, cŭⁿwiⁿtku. In each of the names for *son* there is an "i," and in each name for *daughter* there is an "a" or an "u." These vowels, which appear to distinguish a masculine term from its corresponding feminine, appear in many of the Biloxi kinship names. Thus: yiñqadodi, *his* or *her son's son;* but yŭñqadodi, *his* or *her son's daughter;* yŭñka yiñqi, *his* or *her daughter's son;* but yŭñqa yuñqi, *his* or *her daughter's daughter.* For *his younger brother,* the Biloxi root is soⁿtka; the Çegiha term is isañga; the ꓕɔiwere use iᶜŭñe, and the Dakota have sŭñkaku. *His eldest sister* is tañqi or tañqiyaⁿ in Biloxi, iqañge in Çegiha, itañe in ꓕɔiwere, and tañke or tqañke in Dakota. *Son-in-law* is tondi or tondiyaⁿ in Biloxi, and iqande in Çegiha. *Brother-in-law* or *wife's brother* is tahaⁿni or tahaⁿniyaⁿ in Biloxi, iqahaⁿ in Çegiha, and tahañku in Dakota.

*Marriage Laws.* — A Biloxi man cannot marry his wife's brother's daughter, nor his wife's father's sister, differing in this respect from a Dakota, an Omaha, a Ponka, etc. But he can marry his deceased wife's sister. A Biloxi woman can marry the brother of her deceased husband. Judging from the analogy furnished by the Kansa tribe it was very probably the rule before the advent of the white race that a Biloxi man could not marry a woman of his own clan.

## LANGUAGE.

### PHONOLOGY.

*Alphabet.* — The alphabet used for recording the Biloxi language is that proposed by the Bureau of Ethnology. The following is a list of the sounds occurring in the language: —

a as in *father.*
ă as in *what.*
ā as *aw* in *law.*
b occurs only once, in a proper name.
c = sh in *she.*
d rarely used (see t and q).
dᶿ is a d followed by a slightly audible dh sound approximating the Sanskrit *ddh.*
e as in *they.*
ĕ as in *get.*
f rarely used.
g as in *go,* seldom heard.
h as in *he.*

ɥ a sound heard at the end of certain syllables, but slightly audible, nearer h than q (kh).
i as in *machine*.
ĭ as in *it*.
j as in French, or as English *z* in *azure*.
k as in *kick*.
ʞ a medial sound, between a g and a k.
l occurs only in two modern names.
m as in *me*.
n as in *no*.
ñ before a k-mute, ng as in *sing*, *singer*, but not as ng in *finger*.
ⁿ a vanishing n, scarcely audible, as in the French *bon*, *vin*, etc., occurring after certain vowels.
o as in *no*.
p as in *pen*.
ᑯ a medial sound, between b and p.
q = kh or ch as in German *ach*.
r occurs in one proper name.
s as in *so*.
t as in *to*.
ɟ a medial t, between d and t.
tˢ, a t followed by a slightly audible th (as in *thin*, the surd of dˢ).
u as oo in *tool*.
ŭ as oo in *foot*.
û as *u* in *but*.
ᵾ a sound between *o* in *no* and *oo* in *tool*.
w as in *we*.
y as in *you*.

*Vowel Changes.* — There are instances of gemination, not found in the Dakota, Ȼegiha, etc.: aⁿyadi and aⁿyaadi, *persons;* wadi and waadi, *very;* hapi and haawi, *leaves.* Apocope has been observed, but it demands further study. Doublets have been recorded, usually differing in a single sound. Among these are some in A and O: nawi and nowe, *day;* na-ŭñʞihi and no-oñʞihi, *I wished to do it* (but I could not), etc. A and U: naqoɟodˢa aⁿyadi, *the Alligator People;* nûqwoti, *an alligator;* and nûqodo hedi, *an alligator gar.* A and Aⁿ: ha, naha, haⁿ, haⁿtca, adverbs of time, duration, delay, etc.; -ka and -kaⁿ, forms of the objective ending. A and I: haⁿ, hiⁿ, *when;* qyaⁿ, qyiⁿ, *when;* yanasa, yĭnĭsa, *a buffalo;* qanaqka, qyiniqka, *an otter.* Ĕɥ and Ĭɥ: dĕɥ-towĕ, dĭɥ-towe, and dˢetowe, *to fill it.* E becomes ĭ before qti, *very*, as, tcayĕ, *to expend, exterminate*>tcayĭqti; naskĕ, *long, tall*>naskĭ-qti, etc. E becomes Ĕɥ before ñ: thus, ñʞe or ñʞedi, *I say it*, becomes ñʞĕɥ before ñʞande, *I am, I continue.* I becomes Ĭɥ before d, n, etc.: ni, *to walk*, nĭɥ nedi, *he is walking;* nĭɥ ne kaⁿ, *when he was walking;* adi, *to climb*, adĭɥ de, *he went to climb.* Other vowel changes which have been noted are morphologic in character.

*Consonant Changes.* — At the Washington meeting of this Association in 1891, attention was called to some permutations of consonants which had been observed in the Siouan languages: c and q (sh and kh), x and z (gh and z), ȼ (dh) and n. Since then the following Biloxi permutations have been recorded: c and q, h and k, h and q, p and w, q and qw, q and qy, q and s. The words in which these permutations occur

are not always synonymous, but when we find a word in which c, for instance, is used, we may infer that there is another word in the same language differing from the former only in the substitution of q for c, or that one dialect or language uses c where another employs q. C and Q: cuhi, *a strong odor from meat*, and quhi or qyuhi, *any bad odor;* cuⁿwe, *to whistle*, as the wind does, and iqyuⁿwe, *to roar* or *whistle*, as the wind does; kcicka and kciqka, *a hog;* konicka and koniqka, *a bottle.* H and Q: the roots hăpi and qpi in duhăpi and duqpi, *to remove a hat from the head*, etc. H and K: haⁿ, *and* (connecting verbs), sometimes meaning *when;* kaⁿ, *when in the past* (at the end of a clause). P and W: aⁿsepi, aⁿsewi, *ax;* ayepi, ayewi, *door;* tŭpi, tŭwi, *pail, bucket.* Q and QW: quqĕ, quqwĕ, *the wind.* Q and QY: qa and qya, used as a *sign of past time, customary action*, etc.; qapka, *broad* in body, as a person, *broad* as the lid of a kettle, but, qyapka, *low, near the ground;* qĕ, feminine oral period, etc., qyĕ, one of its masculine equivalents; qeni, qyeni, *but;* qo, qyo, a *sign of the contingent future*, male speaking. This permutation (q, qy) may be compared with the Kansa b and by, k and ky, p and py, as in bĕqliⁿ, byĕqliⁿ; kĕ and kyĕ; and the Anglo-Saxon vowel changes called breakings, as in *cart, ceart, garden, gearden*, in which the "e" approximates a "y." Q and S: yaⁿqi, *the strong odor from a goat;* yaⁿsi, *having a strong fishy odor;* -qti and -sti, *very.* ʞi becomes Q before a K sound, thus, instead of nañʞi kaⁿ, they say naⁿq kaⁿ or naq kaⁿ; for mañʞi kaⁿ they use maⁿq kaⁿ or maq kaⁿ; for e nañʞi kiʞĕ they say, e naⁿq kiʞĕ. Hi becomes Q before a T: aduhi, *a fence;* but aduq tcati, "*a split fence,*" *a rail.* Pahiⁿ, *a bag*, is contracted to paq, in añks paq kidi, *a bullet pouch* (from añksi, *lead*, bullet, pahiⁿ, *bag*, and kidi, *to carry on the back*).

## COMPARATIVE PHONOLOGY.

| BILOXI. | OTHER SIOUAN LANGUAGES. |
|---|---|
| ade, adĕ, to blaze, a blaze. | Çegiha, ane, prairie fire; ꓕɔ., aʇɔe. |
| adi, to climb. | Dakota, adi; Çegiha, ane. |
| ahe, ahĕ, horn, horns. | D., Ç., ꓕɔ., etc., he. |
| aho, ahodi, ahudi, bone(s). | D., hu; Ç., wahi; K., Os., wahü. |
| ama, hama, hamaⁿ, ground, earth. | Ç., maⁿ, majaⁿ; W., maⁿna; H., ama, abwa. |
| amasi, masĭ, iron, metal. | D., maza; Ç., maⁿzĕ; Os., maⁿsĕ, etc. |
| dĭtci, to dance. | H., kidʞci. |

| BILOXI. | OTHER SIOUAN LANGUAGES. |
|---|---|
| duti, to eat. | D., yuta; Ȼ., ȼatĕ; K., yatce; Ɉɔ., ruȝɔe; W., ruto; H., dᵋutᶜi, nutᶜi; T., luta, luti. |
| ha-idi, blood. | H., idᶜi. |
| hapi, etc., leaf, leaves. | D., tcaⁿ-ẇapa; Ȼ., jaⁿ-abĕ; H., midᶜaapa, midȼapa, bidᶜapa. |
| iñ ʞaⁿ, muscle(s). | D., kaⁿ; Ȼ., ʞaⁿ. |
| iqkí-, himself (reflexive). | D., itc'i-; Ȼ., ʞi-; H., iqki-. |
| iⁿstodi, elbow. | D., (Santee), isto; Ȼ., astuhi. |
| ki, kidi, to carry on the back. | D., K., Os., Ɉɔ., k'iⁿ; Ȼ., 'iⁿ; H., ki. |
| ke, to dig. | D., k'a; K., Os., Ɉɔ., k'e; Ȼ., 'e. |
| pa, head. | D., pa; Ȼ., ȡa, nacki. |
| pasaⁿ, ayap pasaⁿ, bald eagle, any sort of eagle. | Ȼ., ȡasaⁿ, bald eagle (only). |
| peti, peti, fire. | D., peta; Ȼ., ȡede; K., pyedje; Os., peʇ·e; Ɉɔ., peȝɔe. |
| psŭki, psŭki, eructate, hiccough. | H., pcuki, eructate. |
| pqaki, sand. | H., puqŭki. |
| tcŭñʞi, dog. | D., cŭñka. |
| tuksiⁿ, armpits. | D., doksi; Ȼ., nusi; K., dusi. |
| wahu, snow. | D., wa; Ȼ., ma; K., ba; H., ma'. |
| ani pupuqi, foam. | H., puqi. |
| tckuyĕ, sweet. | D., skuya; Ȼ., skiȼĕ; Ɉɔ., çku. |
| yani, tobacco. | D., tcandi; Ȼ., nini; K., nanü. |

ABBREVIATIONS.—D., Dakota. Ȼ., Ȼegiha. H., Hidatsa. K., Kansa. Os., Osage. T., Tutelo. Ɉɔ., Ɉɔiwere. W., Winnebago.

## MORPHOLOGY.

It has been said by those to whom we should listen with respectful attention, that in American tongues there is no well-perfected machinery for displaying the relation of the leading and dependent clauses—that true relative pronouns, copulative conjunctions, and cases are unknown—and that in many languages kinship terms and names for parts of the body cannot be expressed without the aid of the inseparable possessive pronouns. I am not prepared to make a wholesale denial of these assertions, but I speak advisedly when I say that they require considerable modification so far as the Biloxi and other Siouan languages are concerned. At present, however, I must confine my remarks to the Biloxi language.

*Vowel Changes.*—E to A: (1) Before dande, the ordinary future

sign, and qo, the sign of the conditional future; (2) before hi or ni followed by a verb, of thinking, desiring, or commanding; (3) before some of the imperative endings, as, te′yĕ, *to kill*, but teya′, imperative; qĕhe′, *to sit*, qaha′, qaha-ta′, and qaha-tĕ′, imperatives; (4) before oⁿni, a sign of continuous or incomplete action, as, de′di, *to go*, da′ oⁿni′, *he is going* (is now on the way). Ĭ and I: ĭ sometimes marks the generic form in the names of some parts of the body, as, ĭsi′, *a foot*, feet (of no particular person); but, ĭsi′-yaⁿ, *his* or *her feet;* ĭpi′, *a liver*, but ĭpi′-yaⁿ, *his* or *her liver;* while i is the characteristic of the second person, as, isi, *thy foot* or *feet;* ipi, *thy liver*.

*Consonant and Vowel Morphologic Changes.* — When the final syllable of a noun or verb consists of h, k, or t followed by a pure or nasalized vowel, that syllable becomes q before tu, the plural ending, as, duti, *he eats*, duq-tu, *they eat;* iñki, *he releases it*, iⁿq-tu, *they release it*, etc.; anahiⁿ, *a hair*, anaq-tu, *hairs*. In relative and dependent clauses, di, one of the verbal endings is omitted, as, dedi, *to go*, but de′ kaⁿ, *when he went*. Kĭdi′, *to have come back*, does not drop its last syllable, as that is part of the root. Instead of the indefinite article, soⁿsa, *one*, is used; and -yaⁿ, appended to nouns, often has the force of the definite article.

*Case-endings.* — Nouns and pronouns have nominative and objective case-endings. These are di or di-ko (pl., tut ko) for the nominative, and k, ka, or kaⁿ, for the objective. Examples: Tcuñ'ḳi-di′ ko ayint′-kaⁿ iyaⁿ′hiⁿ, *the dog loves thee*. Ayin′di ko tcuñ′ḳi-k (or, tcuñ′ḳikaⁿ) iyaⁿ′hiⁿ, *thou lovest the dog*. In′di ko ayint′-kaⁿ iyaⁿ′hiⁿ, *he loves thee*. Ayin′di ko int′-kaⁿ iyaⁿ′hiⁿ, *thou lovest him*. In these examples all distinctions are marked by the case signs or endings, and not by any modification of the verb. Sometimes haⁿ is used instead of kaⁿ to mark the objective case. I have not yet found any dative sign except in connection with the verb, as deyĕ, *to send him, her*, or *it off*, dekiyĕ, *to send off for* another, in which latter verb, ki is the dative sign.

*Forms of Pronouns.* — The ordinary nominative, *I*, is ñḳindi or ñḳindo ko; *thou* being ayindi or ayindi ko, and *he*, indi or indi ko. *I alone*, ñḳindi qya or ñḳintqa. *I too* (*in ordinary speech*), ñḳind-hĕ (of ñḳindi and hĕ). *I too, me too* (used *in complaints*) ñḳindi-qaⁿ or ñḳindi-qyaⁿ, as in the sentence, You should have asked me too, but you did not. Strong improbability is expressed by ñḳindi-qtihiⁿ, followed by naⁿni at the end of the sentence, occurring in such phrases as, How is it possible for me to do it? There are several classes of

possessive pronouns. Of the separable pronouns, ta, *his*, ita, *thy*, iñkta, or ñϡita, *my*, and their plurals in tụ are used after such words as ayek, *corn*, tûwi, *bucket*, and tcụñϡi, *dog;* taya$^n$, *his*, itaya$^n$, *thy*, ñϡitaya$^n$, *my*, and their plurals tatuya$^n$, itatuya$^n$, and ñϡitatuya$^n$ are used after such words as a$^n$ya, *people*, tcụñϡi, *dog*, tohoqka, *horse*, and a$^n$sepi, *ax*. But kinship terms and names of parts of the body take the inseparable pronouns, though in Ȼegiha the names of parts of the body must take the separate pronouns. Sometimes the dative form of the verb follows a noun and its possessive pronoun, as, tcụñ'ϡi ta te'kiyĕ (dog his he-killed-for-him), *he killed* another's *dog;* tcụñ'ϡi i'ta te'hikiyĕ (dog thy he-killed-for-thee), *he killed thy dog;* tcụñ'ϡi iñkta' teq'kiyĕ (dog my he-killed-for-me), *he killed my dog*. A similar usage occurs in the Dakota, Ȼegiha, and cognate languages, but without the possessive pronoun. The Biloxi has the interrogatives kawa, *who? which?* and tcĭdĭϡe, *what?* For the demonstratives *this* and *that* there are several forms. While I have so far been unable to find pure relative pronouns in the Biloxi, I have recorded many examples of them in the Ȼegiha, Kansa, Osage, and ⅃ɔiwere. Correlatives abound in the Biloxi as well as in the other languages of this family. These correlatives are of two classes, correlative adjective pronouns and correlative adverbial pronouns, the former expressing relations of quantity and quality, and the latter those of place, time, and manner or condition. Only one set of these correlatives need be given as illustrations: tca'naska, *how large?* (the root -naske or some approximation is found in Ȼegiha, Kansa, etc.). Answers: te'naska, *this large*, ene'naska, *that large*, e'naska, *that large* (*i. e.*, the size of the aforesaid object); tca'naska ko e'naska, *as large as;* tca'naska ne'di ko uki'-kiñge, *half as large*. *Take as many as you please*, Tcĭna' ayo'yihi ko da$^{n\prime}$ (literally, how-many you-want as take-thou). *He will take as many as he pleases*, Tcĭna' oyi'hi ko da$^{n\prime}$ dande' (how-many he-wants as he-take will).

*Classifiers.* — These words play a very important part in the Athapascan and Siouan languages. In these two linguistic stocks, all objects are classified on characteristics found in their attitudes, the primary attitudes being standing or perpendicular, sitting or curvilinear, and reclining or horizontal. The Biloxi classifiers mark not only these three attitudes, but also the walking and the running; again, they distinguish between the horizontal animate and the horizontal inanimate object, between the sitting animate and the curvilinear inanimate, etc. There are also dual and plural forms of the classifiers. Another

function of the classifiers is to mark continuous or incomplete action. This is effected by placing the proper classifier after the finite verb, as, yao$^n$, *he sings*, yao$^n$ nañʞi, *he sits singing*, *he is singing* (though nañʞi is distinct from qĕhe, the regular verb, *to sit*). The past forms of the classifiers are derived from the present ones by prefixing ka, as, ka-mañʞi, *the one who was reclining* ; ka-nañ'ʞi, *the one who was sitting;* ka-nĕ', *the one who was standing* or *moving* ; while nañʞi is *the one who is sitting*, *the sitting one* ; ne, *the standing* or *moving one*, etc.

*Numerals.* — There is only one series of cardinals, as no distinction is made between animate and inanimate objects. There are multiplicatives, distributives, and numeral adverbs, denoting repetition of action. *Two* and *four* are identical with their respective Dakota equivalents. *One* differs from that numeral in Tutelo only in the initial sound. *Three* closely resembles the Hidatsa and Tutelo forms. *Seven* is formed from *two* and the word for *stock* or *tree*. *Eight* is formed from *three* and the word for *stock* or *tree*. *Eleven* is "one sitting on ten," and *twenty-one* "one sitting on two tens." The numerals are thus inflected: no$^n$pa', *two* ; no$^n$patu', *they are two* ; i'no$^n$patu, *ye are two* ; no$^{n}$'patu, *we are two* ; na$^{n}$'pa-hudi', *seven* ; no$^n$p'-hutu', *they are seven* ; i'no$^n$p-hutu', *ye are seven;* ŭ$^n$no$^n$p'-hutu', *we are seven.*

*Verbs.* — Prior to my second visit to the Biloxi, I had reduced the verbs to fourteen well-defined conjugations, with seventeen examples of double conjugations, and thirty-two verbs which could not be assigned to any known conjugation.

EXAMPLES OF BILOXI INFLECTION, SING., DU., AND PL. SUBJECTS.

| To run off. | To go, depart. | To go home. | To be coming. |
|---|---|---|---|
| S. 3. koqta'di | de'di | kĭde'di | hu'di |
| 2. i'koqtadi | ide'di | yakĭde'di | yahu'di |
| 1. qkoqta'di | nde'di | qʞĭde'di | ñqu'di |
| Du. 3. koqtatu' | | | |
| 2. i'koqtatu' | | | |
| 1. qkoqtatu' | | | |
| Pl. 3. kiqyoq'tu | a'de | ka'de | a'hi |
| 2. i'kiqyoq'tu | aya'de | kaya'de | ya'hi |
| 1. qkiqyoq'tu | ñʞa'de | qka'de | ñʞa'hi |

EXAMPLES OF BILOXI INFLECTION, SING., DU., AND PL. OBJECTS.

To raise him, her, it, S. 3. ksa′wiyĕ (<ksapi); 2. ksawi′-hayĕ; 1. ksa′wŭñɥĕ; Pl. 3. ksa′wiyĕtu′; 2. ksawi′-hayĕtu′; 1. ksa′wŭñɥĕtu′.

To raise them: S. 3. ksowo$^{n}$′; 2. ksowi′yo$^{n}$; 1. kso′wo$^{n}$ ñɥo$^{n}$′; Pl. 3. ksowo$^{n}$-tu′; 2. ksowi′yo$^{n}$tu′; 1. kso′wo$^{n}$ ñɥo$^{n}$′tu.

To give examples of all the known mode and tense signs would occupy more time than could be spared for that purpose on this occasion. But a few of them will be interesting. *I will go* (now — said when in the house), yahede nda dande (now I-go will). *I am going now* (just starting), ede ndedi. *I am going now* (said when met on the way), ede nda o$^{n}$ni. *I was about to go, but I am still here,* ndad ŭ$^{n}$nedi qyeni, eɹiɥĕɥ ŭ$^{n}$nĕ. *I can go,* nde pihe na. *I must go,* nda hi na. *I can go if I choose* (but I am indifferent about it), nde pihedi. *I cannot go,* nda hi ŭ$^{n}$niɥi na. *I ought to have gone* (but I did not), nde pihedidi$^{n}$ ndenina. *I ought not to have gone* (but I went), ndeni pihedidi$^{n}$ ndedi na. *I will go if I can,* nde pihedi qa$^{n}$ (or, qya$^{n}$) nda hi na, or, nde pihe qa$^{n}$ (or, qya$^{n}$) nda hi na. *I will go if it is good for me to go,* nde pi qa$^{n}$ (or, qya$^{n}$), nda hi na. *I will not go if it is bad for me to go,* nde kŭpini qa$^{n}$ ndeni na. *He wants him to go,* da ni kiyûhi. *He* (A) *wishes that he* (B) *could go,* de na′wiɥihi (but B cannot go). *He* (A) *thought that he* (B) *ought to go,* da hi kiyûhi. *He* (A) *thought that he* (B) *had gone, but* (he had not gone), dedi′ wiyû′hi.

Among the many forms of the imperative in Biloxi, the following are the most important, the verb qĕhe, *to sit,* being selected as an illustration: (A) Male or female to a child, qaha! Male to a male, first time, qaha-ta! Male to male, second time, qĕhe-kañɥo! Male to female, qaha-di! Female to male, qaha-tĕ! Female to female, qĕhe-ka$^{n}$! Male or female to children, taa$^{n}$tu! Male to males, first time, taa$^{n}$tu-ta! Male to males, second time, taa$^{n}$-tu-kañɥo! Male to females, taa$^{n}$tu-di! Female to males, taa$^{n}$tu-te! Female to females, taa$^{n}$tu-ka$^{n}$! (B) You too (as well as he): Male to male, qĕhe-ɹakta, you too sit down! Male to female, qĕhe-tûki! Female to male, qĕhe-ɹakte! Female to female, qĕhe-tûki! Male to males, taa$^{n}$tu-ɹakta, ye also sit down! Male to females, taa$^{n}$tu-tûki! Female to males, taa$^{n}$tu-ɹaktĕ! Female to females, taa$^{n}$tu-tûki! (C) *Well, why don't you sit down,* or, *Sit down!* Eɥe qyidi$^{n}$ iqaha hi ko (said after you have been talking some time about so doing). *Sit down yourself* (instead of telling some one else to do it), ayindi iqaha hi ko.

(D) Some of the imperatives of dedi, *to go*, are added: *Well, go* (as you are so persistent), idĕ tĕqti ko dĕd-ki! *You have been so anxious to go home, now go*, yakidĕ tĕqti ko kĭdĕd-ki! *Well, go there and remain* (said when one gets tired of hearing you speak of going), dehande-ha$^n$tca-ta (dẹ, *to go;* hande, *sign of continuous action;* ha$^n$tca, is here *a sign of impatience;* -ta *imperative ending*). *Well, keep on going*, da-o$^n$-ha$^n$tca-ta (da o$^n$ni, *he is going*, said when met on the way).

Verbs in Biloxi, as in the other Siouan languages, are divided into forms which closely resemble the "species" of Hebrew grammarians, though in the Biloxi we find a simple form, a simple instrumental, a frequentative, a frequentative instrumental, a dative, a dative instrumental, a reflexive, and a reflexive instrumental. All these forms do not necessarily belong to each verb. If we take the verb *to wash*, the simple form is dutcadi (verb stem, dutca); simple instrumental, i-dutcadi, *to wash with* (soap, etc., name of instrument being used before the verb; frequentative, dutcatcadi, *to wash it often;* its instrumental, idutcatcadi; dative, kĭdutcadi, *to wash it for* another; its instrumental, if used, would be, ikĭdutcadi; reflexive, iqkĭ-dutcadi, *to wash himself.* Instead of the possessive form of the verb, as in Dakota, Ȼegiha, etc., the Biloxi uses the dative form after the possessive pronoun and its noun. Reduplication occurs in nouns and some adjectives, as well as in many Biloxi verbs. Reduplication in the verb denotes one of two things: (1) repetition of the action in the same place or upon the same object; (2) repetition of the action in different places or upon different objects.

A few examples of the Biloxi modes of predication are given. *I am a Biloxi man*, Tanĕks ñɥa$^n$yadi (from Tanĕks a$^n$ya, or a$^n$yadi, a Biloxi man or person), or Tanĕks ñɥa$^n$yaɟo (from a$^n$yaɟo, a man as distinguished from a woman). *I am a Biloxi woman*, Tanĕks ñɥa$^n$qti (from a$^n$qti, a woman). *Were I in your place* (or, *if it were I*) *I would eat it*, ñɥindi diqyi$^n$, nduti na. *Let me be the one to go* (etc.), ñɥindi di$^n$ nda hi na. *You are the one*, ayĭndi ni. *He is the one*, indi ni. *They are the ones*, i$^n$qtu ni. *Be thou the one*, ayindi diqa$^n$. *Has he been sick since he left home?* ti hu o$^n$ya$^n$ ka-uɥ-ni (literally, house coming in-the-past-that he-has-not-been-sick). *How are you?* tcĭdĭɥĕ ayande (how you-are). *That is a cat*, ktu dan'de (in reply to a question). *He is a man*, a$^n$yaɟo an'da qa$^n$. *He will be a man* (some of these days), a$^n$yaɟo an'da dan'da qa$^n$. *They are everywhere*, yate' yuɥa' qo. *It is his*, indita na. *It is thine*, ayindita na. *It is*

*mine*, ñɥindiñkta na. *It is theirs*, i$^{n}$qtutatu na. *It is yours*, ayi$^{n}$qtu itatu na. *It is ours*, ñɥi$^{n}$qtu ñɥitatu na.

The texts which I have collected furnish many examples of dependent clauses in the various Siouan languages. I have selected, as a fair specimen from the Biloxi, a sentence occurring in the myth of the Duck and her brothers (four Hawks): —

| Eɥeka$^{n}$′ | a′ko | de′yĕ | ha$^{n}$, | "Tcĭ′dĭɥĕ | a′tcu | a′yihiqti′hayĕtu′ | wo," | he′ | yúɥĕ′ |
|---|---|---|---|---|---|---|---|---|---|
| And then | out | made her go | and | How | jerked meat | you (pl.) have so great a quantity | ? | saying that | they were |

| ka$^{n}$, | "Iñkowa′ | i$^{n}$da′hi | o′tu | n′i | e | ha$^{n}$t′ | kiɥĕ′, | "Tcĭ′dĭɥĕ, | yo$^{n}$′ | o′tu | ko |
|---|---|---|---|---|---|---|---|---|---|---|---|
| when | Themselves | hunting | they (fem.) shoot it | say-ing | at length | though | | How | you do | they shoot | as |

| ñɥind-hĕd′ | ya$^{n}$q′kĭko$^{n}$-dăha′," | kiyĕ′ | yuɥĕ′ | kĭde′, | "Eɥeko′, | añks-o$^{n}$′tute," |
|---|---|---|---|---|---|---|
| us too | you do it for us | saying it to her | they were | till | Well | arrows make ye (female to males) |

| kiyĕ′-dăha′ | ka$^{n}$, | añ′ksi | so$^{n}$′sa | o$^{n}$′tu. |
|---|---|---|---|---|
| she said it to them | when | arrow | one | they made. |

In a free English translation this makes several sentences, as follows: They said to her, "How is it that you have such a great quantity of jerked meat?" To this she replied, "They themselves (the brothers) seek the game and shoot it." Then the inquirers said, "Do for us as you have done for them when they have shot at the game." The woman replied, "Well, make some arrows." Then they made a single arrow.

In the third and fourth lines of the sentence just given in the original, dăha occurs at the end of two verbs, being a pluralizing suffix, used to change any person in the objective singular to the corresponding person in the objective plural: for ya$^{n}$qkĭko$^{n}$ means, *you do* or *did it for me*, and kiye, *he* or *she said it to him* or *her*.

Over forty verbs have been tabulated with all the transitions, but as even one of these verbs would fill a page and a half in print, it is best to refer the student to the future official publication for the details.

*Prepositions and Postpositions.* — There are in the Biloxi some prepositions and postpositions: the former, a, *on*; u, *in*, etc., being prefixed to verbs; the latter being placed after the nouns which they qualify, though they perform the functions of true prepositions. These postpositions are of three classes: 1. Generic, in which the places are not fully defined; 2. specific, referring to objects in the distance; 3. specific, referring to objects close at hand. Thus we have ti itka, *in the house*, of the first class; ti itka-ya$^{n}$, *in the house* (-ya$^{n}$, *there, yonder, in that place*); and ti itka-dĕ, *in the house* (-de, *here, in this*

*place*). Another word for *in* is ʞnedi, as, ani ʞnedi, *in the water* (= ani itkayaⁿ, etc.). Peti is *fire*, and peti-kaⁿ may be either *into the fire*, or *out of the fire*, according to the context. *Close to* is yehi or yehikaⁿ, as, ti yehikaⁿ, *close to the house*; tcahămaⁿ yehikaⁿ, *close to the river*. There are three words for *under*, but kuya is the common one: ayahi kuya, *under the bed*; yaqoⁿ kuya or yaqoⁿ okaya, *under the chair*; but, ti yaskiya, *under the house*. None of these postpositions become part of any verbal. But *with* or *to be with* is always inflected in Çegiha, Kansa, Osage, ⅃oiwere, and Biloxi, though it remains unchanged in Dakota.

*Adjectives.* — It has been doubted whether there were any real adjectives in Indian languages. In Biloxi we find several. Thus, waqka, *soft*, as corn: *his corn is soft*, ayek ta waqka; *thy corn is soft*, ayek ita waqka; *my corn is soft*, ayek ñʞita waqka. Su, *bare*, as the feet: *his feet are (entirely) bare, he is barefoot*, ĭsi nati su; *thy feet are bare*, ĭsi nati su; *my feet are bare*, iñksi nati su. Among the participial adjectives we find *broken: broken*, as a limb, is ksâ; *broken*, as the heart, is hâyĕ. *His leg is broken*, yukpĕ ksâ; *thy leg is broken*, iyukpĕ ksâ; *my leg is broken*, nyukpĕ ksâ. *His heart is broken*, yandi hâyĕ; *thy heart is broken*, iyandi hâyĕ; *my heart is broken*, nyandi hâyĕ.

*Adverbs.* — Nati or ti, *entirely, all over* (preceding the modified word) as ĭsi nati su, *his feet are entirely bare*; nati si, *yellow all over*. Nati sometimes means, *in vain, for nothing*. Yaⁿqa, *almost*, follows the qualified word, as in napi yaⁿqa, *almost day*; tahi yaⁿqa, *he is almost dead*; itahi yaⁿqa, *you are almost dead*; ŭñktahi yaⁿqa, *I am almost dead*. Nanteʞe, *nearly*, follows the qualified word, as ite nanteʞe, *you nearly died* (but you have recovered); tcayĕ-dăha nanteʞe, *he killed nearly all of them, he nearly exterminated them*.

*Conjunctions.* — There are several classes of conjunctions in the Biloxi. The copulative used with verbs is haⁿ, which causes the elision of di when that is the final syllable of the preceding word, as pqi haⁿ apûdiyĕ, instead of pqidi haⁿ apûdiye, *he deceived him and* (thus) *repaid him*. The copulatives used with nouns are yaⁿ and yaⁿ hĕ, as, aⁿyaɿo aⁿqti yaⁿ hĕ, *a man and a woman*; aⁿyaɿo yihi aⁿqti yaⁿ yihi hĕ, *men and women*; tohoqk wak yaⁿ ndoⁿhoⁿ, *I saw a horse and a cow*; aⁿyaɿo aⁿqti yaⁿ ahi hamaki, *a man and a woman are coming*. At the beginning of a clause or sentence the Biloxi generally uses eʞehaⁿ, eʞekaⁿ, or eʞaⁿhaⁿ, *and then*. There are two forms of *if*: kaⁿ, having a past reference, and ko, relating to the future. A

clause ending in ko is usually followed by another ending in qo or qyo, the sign of the conditional future, as, Yatanaqti ikikahi$^n$ ko, ita qo, ikikahi$^n$ni ko yande qyaqti qyo, *if you tell about it very soon, in that case you shall die, if you do not tell about it, you shall (in that event) be* (exist) *always.* Ka$^n$ and ko are sometimes adverbs of time, as, i$^n$hi$^n$ ka$^n$, *when it came;* i$^n$hi$^n$q ka$^n$, *when it had to come;* tahiya$^n$ i$^n$hi$^n$ ko, *when his time to die shall come;* tahiya$^n$ i$^n$hi$^n$q ko, *when his time to die must come.*

### SEMASIOLOGY.

As this term is defined as "the science of the development and connections of the meanings of words," it is preferable to sematology, though the latter appears to be more popular.

On page 118 of Duponceau's Memoire it is said, "Should they (*i. e.*, the Indians) wish, for example, to give a name to a certain tree, they think not of designating it by its fruit simply, or by some other unique appearance, but they say, the tree bearing such a fruit, and the leaves of which resemble such a thing."

While there may be tribes in one or both of the Americas that form some of their nouns in this manner, I know that the Siouan tribes, including the Biloxi, do not talk so. In Biloxi the generic term for *tree* is aya$^n$, answering to the Dakota tca$^n$, and the Ȼegiha qɔabe. Of specific tree names in Biloxi I have recorded over two dozen, and only in three names does aya$^n$, *tree* or *wood*, appear as part of the name, and in each of those three the name ends with udi, *trunk* or *stock*, answering to hu of the Dakota and ⅃ɔiwere, hi of the Ȼegiha, and hü of the Kansa and Osage. Thus, the sycamore is aya$^n$ sa$^n$ha$^n$ udi, explained as meaning, *strong wood tree*, but sa$^n$ha$^n$, though sometimes meaning strong, may be a variant of sa$^n$, *white* (as in Kwapa), the sycamore being ja$^n$ sa$^n$ in Ȼegiha, and ɔa$^n$ sa$^n$ in Osage, both meaning *white wood.* The Biloxi call the tree maple the *blue wood tree*, the bark having been used for dyeing blue. The beech is haawudi, *leafy* stock. One species of oak is called "fine," or "small oak," another "the very rough oak," *tree* or *wood* (aya$^n$ not appearing in the compounds).

The pupil of the eye is tutcu$^n$ su supi, "black seed of the eye." A locust is yo sahayi or yo sahedi, "body makes a rattling." The Tar Baby in the myth of the Rabbit and the Frenchman was said to mean, "Man made from tar." But it admits of a closer analysis, thus: su$^n$nito$^n$ni, *tar;* k, a contraction of ka$^n$, the objective sign; o$^n$, *to use* (as well as to do, make, or wear); ha, sign of duration or lapse of time; a$^n$ya, *person, man;* o$^n$ni, *he made.* A pipe is yaniksiyo$^n$, literally, yani,

*tobacco;* ksi, *smoking;* y, introduced for euphony; o$^{n}$, *to use, i. e.*, used for smoking tobacco. The wood duck, *Aix sponsa*, is called taqpa, pt$^{c}$asi, "flat temples." The pelican is "the ancient one that eats crawfish." A ground-hog is called kcicka mayi$^{n}$tka, from kcicka, *hog;* ma, *ground;* y, euphonic insertion; i$^{n}$tka, a variant of itka, *within*—this is probably a modern name. To walk on the ground is mayi$^{n}$ni, from ma, *ground;* y, euphonic insertion; i$^{n}$, locative sign; ni, *to walk.*

About a hundred onomatopes have been recorded, many of which have "he" as the ultimate or penultimate syllable. Hohe, *to bellow;* mahedi, *to whoop;* wahedi, *to cry out, squall, squeak;* quhe, *to roar*, as falling water; ʇuhedi, *to thunder;* sahedi, *to rattle*, as falling corn does; sĭtsĭdedi, *to whistle as a woman does;* sâtsâdedi, *to whistle as a man does.* Many bird names are formed from onomatopes, as, kiskis hayi, "it always says, Kis-kis," the sparrow-hawk.

One of my predecessors, whose works I always read with interest, as I find in them some valuable suggestions, made the following declaration in his Vice-Presidential address before this Section at the New York meeting in 1887: "An American language is usually perfectly transparent, nothing is easier than to reduce it to its ultimate elements, its fundamental radicals. These are few in number, and interjectional in character." I wish that this statement applied to the Biloxi and cognate languages, which I have been recording and studying almost daily for fifteen years; for in that case, at least ten of those years might have been devoted to the investigation of tribal cults, sociology, and other important subjects. However, Dr. Brinton well says that few studies of American languages go beyond the material or lexicographic limit, whereas a thorough study of any language of this class would embrace not only its material, but also its formal and psychologic contents. It will be a very difficult matter to reduce the Biloxi language to its ultimate elements. After the preparation of nearly 3,200 slips for a Biloxi-English dictionary, two months were devoted in 1892 to the arrangement of known and conjectural roots on catalogue cards; but it will require months to complete the investigation. Many of the known roots of the Biloxi show resemblances to the roots of the other Siouan languages; thus, do appears in the Biloxi words for *wet*, names of *parts of the throat, neck; potato;* and *male animal;* the Ȼegiha having nu, the Kansa, du, the Osage ʇu, and the Dakota, do, lo, mdo, or blo, in the corresponding words, with but few exceptions. Ksa (ksa-di, etc.), one of the roots signifying *to cut*, answers to ksa of

the Dakota, se of the Çegiha, and tsûki of the Hidatsa. Many roots in which na, ne, or ni is a syllable, convey the ideas of *bending*, *turning*, or *shaking*. Roots in po and pu convey the ideas of *swollen*, *rounded*. In an article on Siouan phonetic types, yet unpublished, I have collated many of the Biloxi roots with their equivalents in the cognate languages, thus showing the persistence of roots as well as several other important principles.

## FOLK-LORE AND MYTHOLOGY.

*Folk-Lore Notes.* — The Biloxi believed that the spirit of a deer revived and went into another body; and that this could be repeated thrice, but that when the fourth deer was killed the spirit never revived. This is explained as the consequence of the acts of the principal characters in the myth of the Indian and the Deer People.

The Biloxi do not talk about the Thunder being in cloudy weather, because that being is very mysterious. Thunder stories cannot be told except on a fair day.

When the Biloxi see a humming-bird, they say that a stranger is coming. This is in consequence of the action of the Ancient of Humming-birds, as related in one of the myths. They say that the humming-bird always tells the truth.

When the fire crackles, it is a sign of snow or rain. A mutch-hotch pecking on a house is a sign of coming death. If a child steps over a grindstone, its growth will be stopped. No Biloxi will kill or eat a snipe, because that bird, called Ta tci$^{n}$ da hayi, "Always gathers deer fat," was the sister of the Thunder being, and an account of her appears in the myth of the Thunder being.

*Mythology.* — One of the Biloxi myths is that about a tiny frog, called, pĕska, from its cry, "Pĕs! pĕs!" This frog, which frequents streams in central Louisiana, is not over an inch long; it has a sharp nose and black skin. Pĕskana', the Ancient of this species of Frogs, was shut up by his grandmother in order to endow him with superhuman power. Having produced the desired result, partly by means of emetics, she started eastward with him, singing as they proceeded. By and by the Ancient of the Panthers met them, and was thus addressed by the old woman: "This is your sister's son. Look at him and wrestle with him." The Panther, who was very brave, climbed high up a tree, from which he tore off many limbs. Then, springing to the ground, he seized the Ancient of Frogs. But the latter was too powerful for him, catching the Ancient of Panthers by the hind legs,

and whipping him against a tree, thus breaking his jaw in several places. When released, the defeated animal slunk off with a broken jaw. The next animal encountered was the Grizzly Bear, who in like manner was whipped against a tree, which caused his tail to break off near the roots. The third animal which wrestled with the Ancient of Frogs was the Buffalo, who owes his hump to having his back broken when the Ancient of Frogs flung him against a tree. The Deer had his nose broken by the Ancient of Frogs. It was after this fourth encounter that the victor said to the Deer, "I shall remain here under the leaves. When the hunters approach very near, I shall give you warning, saying, 'Pĕs! pḗs!' Then do your best to escape." Therefore when a pĕska cries out now, the people say that some one is about to chase a deer.

In conclusion, let me say that (1) The Biloxi is one of those languages which are noted for their complexity, and not for their simplicity. "It will become obvious to the student of the subject," says Dr. Brinton, "that those American languages which have been lauded for their simplicity are quite sure to be those of which we know very little! . . . Just in proportion as our means of studying them increase, their complexity becomes apparent." [1]

(2) The "ground plan" or "plan of ideas" of the Biloxi differs from those of the Iroquoian tongues and the Athapascan languages of Oregon.

(3) Some of the Biloxi sounds closely resemble those of the Kwapa and Hidatsa languages, the Kwapa habitat being northwest of the later abode of the Biloxi, and the Hidatsa reservation being northwest of the Kwapa territory.

(4) In some of its formal elements, *e. g.*, the modal prefixes of verbs, the Biloxi approximates the Hidatsa and Tutelo languages, and probably the Crow, which may be called a dialect of the Hidatsa; while all the other Siouan languages form a distinct class. As the Tutelo were formerly in the Carolinas, but now have a few survivors (mixed bloods) in Canada, the relative positions of the three peoples may be represented by two triangles, one showing the former habitat, and the other the present one of the Tutelo.

(5) Owing to the persistence of many Biloxi roots which have been identified, the relationship of the Biloxi to the other Siouan tongues has been fully established.

[1] Brinton, *On Polysynthesis and Incorporation*, p. 5, 1885.

(6) On the other hand, the Biloxi differs from most of the Siouan languages in several respects, although only a few examples can be referred to on this occasion.

(*a*) The Biloxi has more imperative forms than the other languages.

(*b*) The Biloxi has fewer correlatives and modal prefixes than any other languages of this stock, except the Hidatsa and Tutelo.

(*c*) Most of the Siouan languages have a quotative sign at the end of each sentence, and frequently the quotative appears at the end of each clause; but in Biloxi none appears except at or near the end of the entire myth, where we find, etu qa, they say or said. In the Ȼegiha, the quotative is suffixed directly to the verb, but in the other languages it is a distinct word.

(*d*) While the possessive pronouns, the dative verbal prefix ki-, and most of the correlatives, resemble in form and sound their equivalents in the other languages of this stock, some of the nominative and objective pronouns show marked variations.

(*e*) The Biloxi and Dakota inflect the nouns man, woman, boy, girl, child, etc., in expressing predication, and the names of parts of the body, in denoting possession, whereas the Ȼegiha, Kansa, ʇɔiwere, etc., make no change in the noun, using after the noun, in one case, a verb to be or exist, and in the other a separate possessive pronoun.

(*f*) The kinship system of the Biloxi is more comprehensive than those of the other tribes, there being over fifty kinship groups, each having its distinct appellation.

(*g*) In the Biloxi there is many a verb which forms its plural or dual by means of a root differing apparently from that occurring in the singular. Nothing like this is found in the Dakota, Ȼegiha, etc.

(*h*) The Biloxi is wanting in the verb "to have," found in the other Siouan languages, supplying this deficiency by certain expedients. Thus, for "I have a father," they must say either, "My-father he he-moves," or "My-father he the-reclining-one." For "I have a mother," they must say either, "My-mother she she-moves," or "My-mother the-sitting-one." Reclining must in these cases be predicated only of a male, and sitting, only of a female.

(7) While it is never safe to rely on the traditions of a single Indian tribe, such traditions have a value when those of scattered tribes of the same linguistic stock point to a common origin; and that value is increased when the evidence of tradition can be supplemented by the evidence furnished by a study of the languages of the respec-

tive tribes. As shown in my paper on the Migrations of Siouan tribes, the Kwapa separated from the Omaha, Ponka, Kansa, and Osage tribes, not later than A. D. 1500, perhaps earlier; yet we note very few changes in the languages. The separation of the Ɉɔiwere tribes, the Iowa, Oto, and Missouri, from the Winnebago must have been prior to 1500, and that of the Mandan at a still earlier period. The Winnebago, Hidatsa, Tutelo, and Biloxi appear to be the four oldest languages of the Siouan family. Whether they were ever offshoots of a Siouan parent speech I dare not affirm, but it is safe to say that the ancestors of the people now speaking those four languages must have dwelt near one another in past ages, when the tribes, as their traditions show, were sedentary, each having a village, which they occupied for a series of years.

The Dakota or Sioux have traditions which claim that they obtained their sacred pipes and other mystic objects from the Winnebago, but I am unable to say whether they admit that they were ever part of the latter people.

After a careful review of the whole subject, I find that, at the very lowest estimate, from a thousand to fifteen hundred years must have elapsed since the separation of the Biloxi, Hidatsa, Tutelo, and the Siouan tribes found by Captain John Smith in eastern Virginia.

# PAPERS READ.

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Order of development of the Primal Shaping Arts. By W. H. Holmes Bureau of Ethnology, Washington, D. C.

Modern science has gone far toward establishing the proposition that the human race, like the various other groups of sentient beings, is the product of evolutional processes, and the student of history has added the corollary that human culture has likewise developed through a long series of progressive stages from infinitesimal beginnings up to the present complex and wonderful conditions. The history of culture cannot, therefore, be complete until the course of its development has been traced back to the remotest beginnings. The arts are the tangible representatives of human progress and achievement, and upon the phenomena of art we are almost wholly dependent for an insight into the initial stages of history. There is, however, a shadowy interval at the very beginning of progress unrepresented by art remains. Into this space we seek to extend our vision by standing on the remotest margins of actual phenomena and prolonging theoretically the vanishing rays of knowledge.

Assuming the general uniformity of nature's genetic processes, we may fairly conclude that in the beginning there was a period of rudimentary or instinctive manipulation of materials during which our race shaped what it needed without premeditation, as the bird builds her nest of sticks and grass, or the wolf burrows a home in the ground. But the time must have come when the hand of this creature man was so developed and his brain so matured that articles furnished by nature, such as sticks and stones, were held in the hand for throwing and striking. These things became implements multiplying the powers of the hand and finally giving man dominion over nature.

The first stage of implement using would consist in the employment of articles furnished by nature. The second stage would be entered upon when the things used began to be modified in shape designedly to increase their efficiency. The step from the first to the second stage would be made possible by unintentional modifications of the primal utensils produced while in use, and the observation of the processes of modification by creatures able to make use of them. This stage would mark the beginning of those manual operations to which we give the name *the shaping arts.* It is these first necessary steps in art, weak and hesitating and almost infinitely slow as they must have been, that more than any others are pregnant with interest to the student of history.

There is little prospect of securing traces of the earliest products of men's hands, as they were probably executed in destructible materials and have long since disappeared. As soon, however, as the shaping operations extended to

stone, permanent records were made, and these are still extant; but the geographic location of these works is not known and may never be known. It has not been determined where man first dwelt, or whether in the primal days he occupied any considerable portions of the world's surface. Early traces of man's presence are said to occur in many parts of the world, but we cannot now determine just how far any portion of these products extends back into primal times.

The nature of man's activities during the prolonged periods of incipient art must be greatly a matter of speculation. But speculation is not necessarily vain. It appears to have in this case sufficient basis in fact and known laws of procedure to make its inferences of interest and value. The general plan of evolution almost universally accepted by scientific men must be allowed to apply to the phenomena of culture. We cannot assume to break the chain of human progress midway by beginning the career of man with the earliest known period of art, which period happens to mark the limit of our positive knowledge. Nature does not regulate her great scheme to accommodate our limited powers of observation. We are compelled to allow that all the phenomena of art have had a development from a beginning more or less remote and infinitesimally small. Some of these phenomena are traced to their inception with ease, the whole course of progress being within the range of historic observation, as with the art of using steam and the art of photography; but others reach far back into the unknown past, and we seek the means of extending our vision that their genesis may also be understood.

As a first step in the consideration of this subject, it is necessary that an analysis be made of those groups of art phenomena that extend farthest back toward the beginning of culture. We shall thus learn something of their relations to the stages of progress, and acquire some definite conception of antecedent conditions.

*Categories of Phenomena.* — There are several categories of phenomena pertaining to the shaping arts, a few of which are of prime importance in this investigation. Classification groups these phenomena as follows: —

1st. By the materials employed in art, as mineral, vegetable, and animal substances; as stone, clay, metal, wood, bark, fibre, bone, horn, and ivory.

2d. By the shaping acts or processes employed in the production of objects of art, as fracturing, pecking, grinding, cutting, modelling, and building.

3d. By the function or use to which the products of the shaping arts are devoted, as for food acquirement, defence, shelter, and transportation; as for cutting, piercing, breaking, grinding, and polishing.

4th. By culture stage, as savage, barbarian, and civilized; as palæolithic, neolithic, and metallurgic.

5th. By time periods and order of development.

6th. By the peoples concerned in their manufacture and use.

*Classification of Shaping Acts.* — The consideration of this entire series of categories is impossible in this place, and I propose to review but a small portion of the field, — that relating to the operations of shaping stone in the initial stages of art. This paper then becomes a study of the early history of culture from the point of view of the processes of shaping stone.

A glance at the accompanying synopsis will convey a clear notion of the

position of the group of phenomena here to be considered, with respect to the whole field. The shaping arts are divided primarily into manual and chemical groups. The first includes all those things shaped directly by the human hand aided by mechanical appliances; the second includes those in which the manual operations are assisted by chemical agents, such as heat, acids, and electricity.

The manual arts employ mainly six groups of processes, to which I have given the names fracturing, bruising, abrading, incising, modelling, and constructing. Four of these groups — the four placed first in the synopsis — are concerned in our studies of the earliest culture, and pertain to the shaping of stone in its elementary utilization.

SYNOPSIS OF THE SHAPING ARTS.

- Shaping arts
  - Manual arts
    - 1. Fracturing — Splitting, breaking, flaking, etc.
    - 2. Bruising — Battering, bruising, pecking, etc.
    - 3. Abrading — Grinding, rubbing, polishing, etc.
    - 4. Incising — Cutting, incising, piercing, etc.
    - 5. Modelling — Moulding, stamping, hammering, etc.
    - 6. Constructing — Building, weaving, sewing, etc.
  - Chemical arts
    - 1. Heat fracture.
    - 2. Explosion fracture.
    - 3. Etching.
    - 4. Accreting, etc.

*Origin of Manual Processes.*—Taking the evolutional view of the development of man and his arts, we must first turn our attention toward the probable activities of the creature man as he issued from the pre-anthropic stage and began slowly to make use of the objects with which he was surrounded for implements and utensils. By the utilization of stone in the form of fragments, nodules, and boulders, the properties of that material would be gradually revealed to him, and in time four processes of modifying its shapes would inevitably be suggested and utilized. These are fracturing, bruising, abrading, and incising.

*Fracturing Processes.* The fracturing processes are placed first for reasons that will appear in the sequel. As known to us, they employ two groups of

acts, percussion and pressure. The first of these implies the use of a hard and heavy implement with which the stone to be shaped is struck, producing fracture; the second implies an implement of at least moderate hardness which is pressed against the brittle stone, producing fracture. These necessary acts — the simple manual operations — are so elemental as to be within the reach of man in a very low stage of mental and physical development. The first, percussion, probably the only act employed in the auroral days, demands nothing more in the way of skill than that required in the casting or striking of one stone against another, or that required in the cracking of a skull or a nut. In the operation of this process a hard, compact hammer of stone or other suitable material having a convex striking surface is essential. The second process, pressure, is less primal, requiring before it can be operated with success a specially prepared tool of hard wood, bone, or other compact substance. The several varieties of acts employed in fracturing are named, according to the nature of the particular results produced, *breaking, splitting, flaking*, and *chipping*. The term flaking is in common use to represent the form-elaborating operations of the group. The material shaped must be measurably compact, homogeneous, and brittle. Such stone is widely distributed over the habitable world.

*Bruising or Battering Processes.*—The acts employed in this class of operations are wholly or in the main percussive, the impact resulting in a bruising and crumbling of minute portions of the surface of the stone. The hammer employed must be hard and tough, and the stone shaped must be sufficiently tough to practically preclude fracture by the ordinary blow. The simple act, like that required for fracture, is quite elemental, and within the reach of a creature of low organization. No specialized tool is necessary, the result being reached by striking one stone against another of proper relative durability. The several acts are known as battering, bruising, and pecking; the last term being in common use for the act by which shaping is mostly accomplished. Materials suitable for shaping by this process are plentiful and very generally distributed.

*Abrading Processes.* — Shaping by abrasion in its most elemental form results from the rubbing of one object against another with such force as to remove minute particles from one or both. The operations are generally expressed by such terms as grinding, rubbing, and polishing. All stones are abradable, and most stones can be made to serve in the active operations of abrading. The act is so simple that it may be performed by any creature having power to grasp the rubbing stone. Its employment in the shaping arts was undoubtedly primal, although it may be hard to secure tangible evidence on this point.

*Incising Processes.*—The incising acts are also simple in their nature. In their most primal form they are practised by all creatures having teeth and nails. In art they include the shaping of materials by *cutting, piercing, picking, scraping*, etc. They imply the use of a hard edged or pointed tool and a substance to be shaped somewhat less hard. Though a primal art, it is doubtful if incising was largely applied to the shaping of stone in the earliest times. This appears from the permissible assumption that stone soft enough to be cut and scraped would not be required in the simple acts of food getting and defence, and the making of vessels, pipes, ornaments, and ceremonial stones did not form a part of the accomplishments of those days.

There are a number of well known operations that combine one or more of these processes, or that pass imperceptibly from one into the other. Cutting and drilling often combine the bruising with the incisive methods. Sawing may be done with an abrading edge or with serrations that incise. Boring is likewise accomplished either by cutting or by abrading points and edges.

From this brief analysis of the four simple primal shaping acts, and a consideration of their relations to the mental and physical powers of auroral man, as well as to the available materials of his environment, I believe it impracticable to reach any conclusion as to which of these acts would first be consciously employed and intelligently and generally utilized in shaping stone. But there are other criteria which may assist us in the attempt to place them in their proper sequence and relations to culture progress.

*Order of Arts dependent on Men's Needs.* A study of the elemental shaping acts does not seem to aid us in determining what particular art would take precedence or what variety of art product ought to characterize the earliest periods of human history. If, however, as appears to be the case, the four shaping processes were equally within the reach of man when art began, it does not necessarily follow that all would come into use at or even near the same time or stage of advancement. It is not the simplicity or discoverability of a shaping process that decides the order of its adoption. The question is rather as to whether or not it is better suited than any other process for supplying human needs. The simplest process possible, though in operation before man's eyes from the beginning to the end of a career, would never come into use did it not subserve the requirements of existence. The proposition may be safely made, that, capacity and environment being uniform, the shaping process that would directly supply a permanent or constantly recurring need not otherwise supplied would be the first process utilized.

A study of human needs in the auroral days may assist in throwing light upon the order of succession and course of development probably taken by the implement-producing arts. We may inquire what devices would naturally be called for in supplying primal needs; first the need of food, second the needs of defence and offence, third the needs of shelter and clothing, fourth the needs of transportation. The need of food is a first and ever present incentive to action, and in early periods men's ingenuity must have been constantly exercised in securing a sufficient and permanent supply. Food getting would lead to the development of varied activities, and call into use all available manual aids. It would certainly in time lead to the multiplication and specialization of utensils, thus opening the way for progress in the shaping arts and the evolution of culture.

It is necessary, then, to inquire as to the probable nature of the artificial devices that food getting would call into existence. The devices employed would depend on the nature of the food resources available to primitive man. The question is complicated by the fact that environment is far from uniform, and that the food resources vary with the habitat. Yet, considering general conditions only, we are able to reach measurably satisfactory results. Whatsoever man's habitat, his food resources were limited to the products of animal and vegetable life, or to both combined, and, so far as the use of stone is concerned in dealing with these substances, it is safe to say that two classes of implements

and only two would be in constant demand. First, roundish or blunt stones would be needed for throwing, striking, crushing, breaking, grinding, etc.: second, sharp or incisive stones would be demanded for cutting, piercing, digging, scraping, and the like. The same statement may be made with respect to the stone tools applicable to purposes of defence and offence, and available in activities pertaining to shelter, clothing, and transportation.

These two general classes of stone implements fulfilled, so far as stone could fulfil them, all the requirements of man's existence in primal days; and if the question were limited to that of the relative need of blunt and sharp stones in the practice of the arts, we should be compelled to say that no distinction could be made, that one class could not claim precedence over the other in usefulness or in period of utilization.

*A Question of supplying Wants not otherwise supplied.* — But it should be most carefully noted that the question is not one as to the comparative usefulness of these forms of implements, or even of the period of their adoption, *but of their production as works of art.* Which form would man first be induced to shape for himself, thus adding a group of artificial utensils to his simple list of mechanical appliances? If, as seems to be the case, both classes of tools, the blunt and the sharp, are equally essential to man, the question becomes one of natural supply. If nature furnished all that was required in the way of tools, art would not be called in to produce them. If nature supplied one class meagrely and the other abundantly, the meagre class would be added to by artificial means. Now if we review the various regions of the world that could have served as the abiding place of auroral man, we find that the rounded stone — the breaking, bruising, grinding stone — is nearly everywhere more readily obtainable than the cutting, piercing stone. The former, being ready at hand, would be at first most freely utilized and for a long time utilized in the natural state, while the latter, being also known and used, yet comparatively rare, would be artificially produced as soon as the capacity to do so was developed.

The artificial sharp stone, the intentionally shaped sharp stone, would thus naturally have precedence as an art form over the intentionally shaped rounded stone. It would probably be the first representative of the shaping art in stone. But there are other points to be considered.

*Operation of the Primal Shaping Acts. — Incipient Stages.* — We must now look more fully into the operation of the four elementary stone shaping acts, — into the beginnings of the arts to which they give rise. It is important to note that the act, the essential element of the process, is not necessarily an index of the simplicity or ease of its utilization. The ease of the first step in a long and tortuous pathway does not determine the ease of the journey. The ease of the first shaping act does not determine the ease of operating it in such a way as to produce a desired and final result. We observe that in art a desired and definite result may be obtained by a *single* shaping act, or that a *succession* of acts may be required. It is also clear that the acts may increase in difficulty as the operations proceed. The intelligence that directs a first act to secure a direct result may not be equal to the task of directing a series of acts, howsoever simple, aiming at a remote result. In general it may be said that a single-act result would be the first designed result reached in the shaping arts. A two-act result would follow, and would precede results that depend upon ten, or twenty,

or a hundred, or a thousand acts. Let us examine the four primal stone-shaping processes, fracturing, bruising, rubbing, and cutting, with respect to this point. What is each capable of accomplishing under the simple, elementary conditions that must be assumed for the incipient days of mind and art? Of the four processes, that which produced an immediate, palpable, available result would be first utilized. The fracturing act, the blow upon a brittle stone, would beyond all dispute be that process. Such a blow produced at once one, possibly two, keen-edged tools of forms admirably suited to the common and ever present needs of the man who must rend flesh, dress skins, cut wood and bone, and dig roots.

On the other hand, the bruising blow, the shaping act by means of which tough stones are shaped, produces an almost imperceptible effect on the stone struck; there is no suggestion of a useful result,—a result that could add to the availability of ordinary natural forms. The nearest distinctive result is far away and obscure, and withal, even when reached, would not be measurably superior to the forms freely furnished by nature.

The dullest mind would be able to understand and utilize the simple fracturing act; a mind keen and far seeing would hardly grasp the nature and possible results of a process so obscure as that of bruising or pecking a piece of rock into definite and unaccustomed shape. This is well illustrated by the almost total failure on the part of students of archeology to understand the operation of the process in its details until elucidated during the last year by Mr. J. D. McGuire of Washington. The cultural distance between the practice of the two processes, flaking and pecking, would represent in all probability a considerable period of progress.

*More Advanced Stages.*— The operation of the shaping processes may be still more fully analyzed and surveyed with relation to actual known implements. The brittle stone, to be more than simply fractured, must be held in the hand and struck with another stone. The stone to be bruised into shape must also be held in the hand and struck with another stone. The positions may be the same, the shapes the same, and the act the same. The brittle stone is struck and is broken, producing perhaps a cutting or piercing tool. A second blow produces a second tool, and also modifies the shape of the stone held in the hand. A two-blow tool has thus been made in shaping one-blow tools. By the time ten tools or flakes have been made, the portion held in the hand has been shaped by ten blows not directed to its own development, but shaping it adventitiously as a nucleus or core. The results are so well defined and tangible that they could not escape observation, and further experiment would be encouraged. Skill to accomplish soon follows where wants direct the effort, and tangible results are at once attained. From the initial steps of intentional flaking the way would be always open to the achievement of higher and higher results. Advancement could not, however, be rapid; wants had to develop, conceptions ripen, skill increase, and methods differentiate by infinitesimal increments, and the highly specialized flaked implement is farther away from the first designed stroke in flaking stone than the printing press is from the well specialized flaked implement of savage days.

On the other hand, again, the hard, tough stone fitted for elaboration by pecking is struck by the hammerstone, and the only result is a slight crumbling of

the surface,—a little white dust. There is no suggestiveness, no innate germ of progress in this result, and there is apparent to the primeval operator no reason why such a blow should be repeated, and even if some blind impulse should lead on to the striking of a hundred blows, no measurable progress would be made toward any tangible result, for a definite conception must be in the mind and a clear notion of how to realize it as well, before such result would be possible. The first step neither yields a result nor opens the way to a result. The first step is in the dark,—in pathless darkness. So far, then, as the process of itself is concerned, it stands little chance of primal utilization as compared with the flaking process.

But is it possible that inspiration as to the utilization of pecking could come from outside sources, from conceptions engendered by the simple operations of food getting and preparing? In cracking nuts or pounding seeds, (for these must have been among the primal activities,) the stones employed would through wear finally exhibit slight concavities. The stones used in the hand would also be modified in shape by striking and rubbing. Could such suggestions possibly give rise to the independent use of these operations in shaping implements of stone? It is not quite clear that the shaping accomplished in the mere routine of use would suggest to the very simple mind the idea of shaping in the abstract, for the shaping in use was adventitious and not necessarily intelligible. It seems likely that man would go on indefinitely using what nature and advention supplied unless there was some positive practical suggestiveness in the technical results accomplished. Certainly the tedious pounding and abrading processes blindly operated in food preparation would stand little chance of being applied, in primal days, to the shaping of tools and utensils of specialized shapes and uses, and especially to the production of implements with sharp points or cutting edges.

The natural tendency of the pecking blow is to blunt and destroy all edges, and the process would have to be diverted from its natural channels by strong forces to make it produce anything like an edged tool; the conception of such a use would have to be acquired by familiarity with edged tools of other classes and materials. The celt, gouge, and grooved axe are the principal implements, made by pecking and grinding, in common use among savage peoples. These cannot be primal forms, as they represent ripened conceptions, specialized technique, skilful manipulation, and highly differentiated uses and methods of use. They are practically without ancestry in their own line. Altogether there seems to be little or no art produced by the pecking and grinding processes that could be safely assigned to primal times. An examination of tools of these classes reveals the fact that in very many cases the processes supplement that of flaking, and it is not impossible that they were first brought into notice and use as a means of getting rid of irregularities and excrescences commonly resulting from imperfect fracture. Pecking would inevitably be suggested in the progress of flaking operations, first, by the effect on the hammer-stone, which is modified and specialized by repeated contact with the stone flaked; second, by repeated efforts to remove flakes where the stone happens to be especially refractory. The repeated blows bruise the stone, modifying its shape and suggesting the possibility of shaping by this means. The abrading processes might also be suggested in similar ways, and especially by the use of flaked tools in operations which modified and polished their edges.

Both the pecking and rubbing processes are especially adapted to elaboration and finish, and are poorly qualified to deal with shapes not already approximate. They did not attain their highest usefulness until superstition and æsthetics became factors in art, encouraging elaboration of form and delicacy of finish.

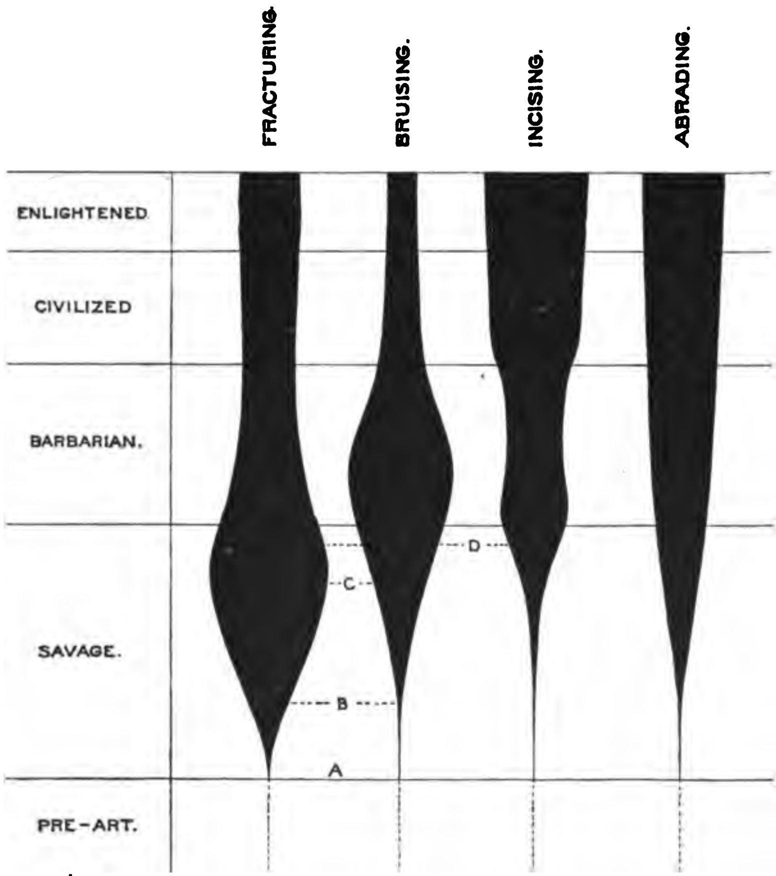


DIAGRAM OF RELATIVE PROGRESS.

I have constructed a diagram to express, in the most general way, my conceptions of the probable relationships of the four shaping processes to the stages of culture progress. The accumulation of additional data will in time enable us to express these relations more fully and with more certainty, but the task is beset with difficulties, for the reason, mainly, that the origin and progress of these arts are not uniform among all peoples. The genetic columns can at best but express generalizations, and are largely theoretical.

The column representing the history of *fracturing* arts, so far as it relates to the earliest times, is based on the observations and inferences already presented. The flaking act was a primal act, and the dotted line descending into the pre-art stage indicates this. On crossing the line — or soon after crossing the line — separating the pre-art from the lowest art stage, I assume that the act was first utilized in the art sense, and that progress began. Being a simple act, and constituting, as operated in fracturing stone, a simple process giving immediate, tangible, and available results, I conceive that its use would increase rapidly through early savage times, dominating the stone-shaping process of that period and culminating in late savage times. The employment of the group of processes developed from the simple fracturing act probably decreased to a considerable extent in barbarian times as other processes came into prominence, but it has continued in active use, especially in quarrying and roughing out stone, for all classes of works, architectural, sculptural, and miscellaneous, up to the present day.

The second column is intended to represent the development of the arts which shape stone by *bruising* and *crumbling* its surface. I have already explained why the process in its simplest form may be considered primal, as having its origin in the pre-art stage of man's history. Its use in shaping must have been suggested to man at a very early stage of art development, and the lines of the diagram are allowed to expand gradually throughout the savage stages of progress. Observing the obscurity of the effects of the bruising act, the long series of operations necessary in producing the simplest art form known, and the comparative rarity of pecked implements that would fitly characterize the beginning stages of culture, the lines have been made to diverge very slowly at first, parting rapidly in barbarian times, during which pecked stone seems to have taken the lead among many peoples as a shaping process. The process in its purity appears to have fallen somewhat into disuse in civilized and enlightened times, the acquirement of hard metal tools having given incisive methods a very decided advantage. The fact is, however, that the shaping of hard stone by means of metal chisels partakes of the nature of a compromise between the cutting and bruising processes.

The active principle of the *incising* arts must have come up with man from the state of nature as distinguished from the state of art; but the development would be slow, on account, first, of the absence of hard cutting tools, and, second, of the absence of stone that could be cut with ease into useful forms. An expanding is indicated in late savage times, where it is assumed peoples began to use soft stones for vessels, ornaments, and ceremonial articles. The fact that the soft stones had, as a rule, to be quarried, probably retarded the development of this process. Again, when hard metals came into common use in late barbarous times and in early civilization, cutting stone took a prominent place in the arts, and has never since yielded its ground.

The history of the *abrading* processes is a very interesting one, but the lines indicate few vicissitudes in its progress. Beginning near the threshold of art, it advanced but slowly, serving mainly as an auxiliary to the other processes, being devoted especially to finish and beautification.

*Influence of Environment.* — In discussing a scheme of evolution for the shaping arts, I have assumed what I conceive to be average conditions of environment;

that is to say, an environment where all ordinary materials are present and available in prevailing proportions. It is apparent, however, that determinations based on such an assumption, even if correctly made out, may not agree with the actual order in the earliest development of art. The environment of the first group of men may have contained all the ordinary elements of stone art, or it may have been without one or more of these elements. If it did not contain varieties of stone suitable to each process, then there would be a disagreement between the theoretic order as here worked out and the real order.

But the race may have been scattered over a wide region at the period of the birth of art, separate groups having distinct ranges of mineral resources. Great diversity of art conditions would thus arise. The group deprived of brittle stone would develop its stone art — no doubt very slowly — through the bruising, grinding, and cutting process, and flaked stone would be practically unknown. The group having only brittle stone would have but meagre traces of pecking and cutting operations, and flaked art would have full sway. To complete the study a separate culture chart would have to be constructed for each group of isolated peoples; for the flaked stone age of one would occupy the position required for the pecked stone period of the other. But the lines between mineral regions are not usually hard lines, and communities of men howsoever primitive are not fixed in habitat. Arts change with change of place and consequent change of environment, and, taking the sum total of the conditions under which a set of groups of men would live, the mean result must, it seems to me, correspond somewhat closely to that expressed in my scheme. Altogether it would seem to be a remarkable condition of events that would permit either of the stone-shaping processes to exist in any considerable degree of development without being accompanied by traces of the others. It would certainly require very exceptional conditions to produce such a result.

Although expressing the view that the exclusive use of a single process or group of processes for a long period seems improbable, I do not wish to antagonize the idea of a flaked stone period in Europe. My diagram allows for such a period covering the space from A to B. That such a period should exist in its purity, however, until the highest flaked forms were developed, as to C, and until a graphic art equal to the realistic delineation of men and animals by engraving on bone, say to D in the incising column, should exist and flourish, is, I am compelled to say, in view of the considerations brought forward in this paper, hardly less than extraordinary.

*Conclusion.* — I am fully conscious of the fact that this brief study is but a rough outline of a part of a noble subject. Prolonged investigation and profound study are essential to the completion of the work laid out. I have sought theoretically a means of approaching and examining that part of primeval history not within the range of other ordinary lines of research. Through an analysis of the elementary shaping processes, — the agencies by means of which man gained his sway over nature, — I have undertaken to determine the order in which these operations would probably originate and develop, and thus to place the varied art products to which they give rise in their proper relations with the successive stages of unwritten history.

Such studies cannot add greatly to our actual knowledge of events, although they may serve a good purpose in confirming or discrediting conclusions reached

by other means; but they will materially assist in preparing the way for an intelligent consideration of those meagre shreds of history that extend, like the edge of a frayed garment, back into the realms of the unknown.

The results of the present study seem to suggest the need of conservatism in interpreting the scattered records available to prehistoric archæology. Where the conditions under which men have lived are so varied, there must needs be great diversity in art achievement, and the order of events established in one region cannot, notwithstanding strong tendencies toward uniformity, be applied with safety to another or to all. Regional art groups must be examined primarily in the light of their own evidence, and general results are to be reached by a comparative study of these special results. The true, the actual order of progress of the race in the primal — the pre-archæologic — stages can never be absolutely known, and thus it is that hypothesis is appealed to to supply an order of events consistent with what is known of the laws of life and art.

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## Caches of the Saginaw Valley, Michigan. By Harlan I. Smith, Saginaw, E. S., Mich.

The entire territory draining into the Saginaw River, and along the shore of Saginaw Bay, is rich in traces of a considerable habitation by a people previous to the coming of the earliest white settlers, the French traders and Catholic Fathers. Workshops where implements were made, village and camp sites, burial grounds, burial mounds, enclosures and embankments for defensive purposes, pits for the storing of provisions, etc., and caches or hoards of blades, have all been discovered in this locality, while the surface is strewn with objects made or used by man at a time before the advent of the whites. Chippewa traditions refer to this locality as a favorite hunting ground.

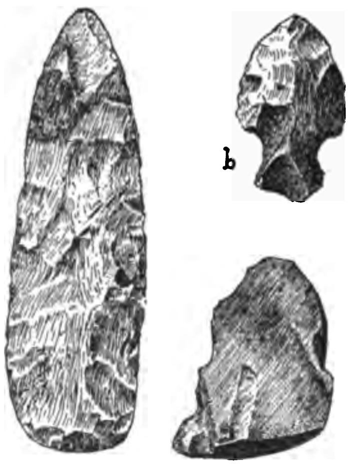
Among the remains of most interest at this time, when quarry refuse and workshop materials are receiving attention, are the caches or deposits of chipped blades of chert. Nine of these caches have been discovered of which records are kept. How many may have been ploughed out or discovered through various methods, and scattered without even a mention, is not easy to estimate. But that many more of these interesting hoards of the treasures of a primitive people will be found in the valley as further explorations are made, I feel confident. The blades found in caches were perhaps made at the quarries and transported to the villages there to be stored or buried in moist earth, which kept them in a workable condition, where they could be easily obtained and worked up into the various specialized forms as such implements were required for use, or they may have been used as they were without specialization. The transportation might have been by canoe in this locality, since all the caches as yet found have been near navigable water; and the material of which the blades from the majority of the caches were made is thought to be from the chert nodules of the sub-carboniferous series, which outcrops in a circular line, along the shore of Saginaw Bay near Bay Port, through the headwaters of the Cass and Shiawassee Rivers, at Jackson, etc. This outcrop is deeply covered by glacial drift throughout much of its extent. Where the rivers cut through the glacial débris

and along the bay shore the rock could be readily quarried, and all of these places are in water communication with most of the village sites of the valley. The blades from eight of the caches were of a material closely resembling the chert nodules of the sub-carboniferous series.

For convenience the caches have been named, and thus associated with the locality where they were found or with the finder.

A general description of these nine caches may be of interest.

1st. The Golson Cache No. I. (124) consisted of 83 pieces, which were found about four feet below the surface at a large village site on the west bank of the Saginaw River, just below the junction of the Tittabawassee and Shiawassee Rivers. Chippewa traditions state that in prehistoric times a great village of another people was located at this place, and that the neighboring tribes, including themselves, united, invaded the valley, exterminated the inhabitants, and took possession of the land. The Green Point Mounds are two large conical mounds at the western side of the village site in which, it is related by the Indian traditioners, a part of the exterminated people were buried. The locality is rich in stone and pottery remains, and more bone implements have been found upon the surface here than in any other place in the valley. The chipped blades found in this cache were made from concretionary chert nodules. The blades are oblong, and about two inches in length. Some are of the "turtle-back" shape. The cache was discovered by Edward S. Golson while co-operating with the author in a preliminary examination of the village site. It is now in the Peabody Museum at Cambridge, Mass., to which institution it was presented, and was the first complete cache to be received by the Museum.




a c

SPECIMENS FROM GOLSON CACHE No. II., $\frac{2}{3}$ size.

*a*. One of the largest blades. Others were of a similar shape, and graduated in size from this to about the length of *b*.

*b*. The blade specialized by notching.

*c*. Chip, presenting from base to point an appearance identical with a chert nodule from outer surface to interior.

2nd. Golson Cache No. II. consisted of 59 blades, found about one foot below the surface at the village site on the eastern side of the river directly opposite the cache just described. The implements of this cache are leaf-shaped, average about 1¼ inches in length, and are of chert. One of the blades had been specialized, by notching, at the base.

The points of two of the blades of the cache were broken off and missing while two of the blades were broken into two parts.

3rd. Frazier Cache No. I., consisting of over 800 pieces, was found about one foot below the surface at a very large village site on the south side of the Tittabawassee River near Paine's Station, about five miles above Saginaw. This village site is also mentioned in the Chippewa traditions as being a large village, captured by the invading force. The Frazier Mound, a very large mound which contained many human skeletons, is supposed to have been the burial place of the unfortunate people. This mound has been entirely removed for the commercial purpose of obtaining the sand of which it was constructed. In the cache, which was located within a few hundred feet of the mound, were found four varieties of blades. First, large, black, leaf-shaped implements, about eight inches long, having a very delicate stem formed at the tip of the base by two notches, and showing concentric markings. Second, similar implements about three inches long, showing concretionary structure very plainly ; the centre being black and hard, the tips grading off by successive rings to a comparatively soft yellowish chert. Third, small yellow chert cache forms, evidently for specialization. Fourth, a few of the latter specialized by notching.

TYPICAL SPECIMEN OF THE FIRST VARIETY OF BLADES FROM FRAZIER CACHE No. I., ⅔ size.

4th. Frazier Cache No. II. (226) consisted of one large, black, leaf-shaped implement, similar to those of Frazier Cache, No. I., surrounded, I am told, by thirteen rubbed stones. This curious cache was found about one foot below the surface a few feet from Frazier Cache No. I.

5th. The Merrill Cache (125), consisting of over 100 blades, was found about one foot below the surface, near the south bank of the Tittabawassee River, about four miles above Saginaw. The specimens from this cache were of concretionary chert, resembling closely that found at the modern quarries at Bay Port, Michigan.

6th. Cass Cache No. I. (122), consisting of about seventy blades and a quantity of chips and flakes, was found about eight inches below the surface, near the south bank of the Cass River, at a point some three miles above Bridgeport. The material of which these specimens were made was of a dark blue color, and entirely different from the chert found in the other caches. The blades were leaf-shaped, and about two inches long.

7th. Cass Cache No. II. (126), consisting of twenty-two blanks and twelve pieces of nodules of chert, very similar to that of the sub-carboniferous outcrop, was found just below the surface of the soil, near the south bank of the Cass River, at a point about four miles above Saginaw. The twelve pieces of raw material lay in a pile, and the twenty-two blades were spread out near them. Chips and flakes were abundant near the cache. It is possible that this was a workshop, the raw material being piled in one place and the worked rock placed near at hand. Some of the blanks were of the "turtle-back" shape, while others had been worked on both sides. Some of the pieces of chert had been broken off in such a way that one side was flat, while the other presented the rounded outer surface of the concretion. From this form it is but a short step to the "turtle-back" form, and then to the blade having both sides worked by secondary chipping.

8th. The Wille Cache (123) consisted of two celts and about one hundred and seventy-five chipped blades of a triangular shape, averaging 1½ inches in length. This cache was found in a small marsh hole or periodical pond near the north bank of the Cass River, about opposite Cass Cache No. II.

9th. The Bay Port Cache (127), consisting of one cross section of a chert nodule and forty-seven "turtle-back" blanks, was found two feet below the surface in a muck jungle about one hundred feet from the shore of Wild Fowl Bay (a branch of Saginaw Bay), at Bay Port. The specimens of the cache were found in a row lapping over one another, in a manner reminding one of shingles on a roof. There is little doubt that the material of which the specimens of this cache were made was obtained within a mile of the spot, as there are within that distance numerous outcrops of sub-carboniferous rocks bearing concretions, the material of which is similar, in appearance at least, to that of the cache specimens.

In both the Cass Cache No. II. and the Bay Port Cache there is a peculiar blade having a straight bevelled edge on one side. It seems probable that this was formed by flaking the pieces for turtle-backs from a round concretion. The first one removed would be perfect, but after that, if the material was used without waste, each piece flaked off would have one side bevelled where its predecessor had been removed from the nodule. The specimens in these two caches were more crude, and this bevelled edge shows, while those in the other caches were further developed, and the traces of the bevel, if it existed, are wholly removed. From the relation of this material to that of the rock outcrops of easy access to the village sites where these caches have been found, it would seem that the aborigines had quarries in the valley, which it is hoped may soon be discovered and studied. That the nodules of raw material were transported is certain, as they have been found in Cass Cache No. II. far from any material of similar nature. Perhaps of most interest, however, is the large number of caches found in the limited territory.

THE RESULT OF EXCAVATIONS AT THE ANCIENT ARGILLITE QUARRIES, RECENTLY DISCOVERED NEAR THE DELAWARE RIVER ON GADDIS RUN. By H. C. MERCER, Doylestown, Pa.

THE study of the ancient argillite quarries at Gaddis Run, Bucks County, Pennsylvania, discovered May 22, and bearing directly on the problem of glacial man in Eastern North America, has seemed of great importance, because these quarries, unlike the jasper mines in the Delaware Valley, recently proved to be the work of modern Indians, are workings by an ancient people in *argillite* (metamorphosed slate with conchoidal fracture), the same stone with which numerous observers assert that man living on the Lower Delaware at the time of the melting of the great glacier made his rude implements seven to ten thousand years ago.

The case seems to demand that we should assume nothing from other work, but, going over again the well known points of quarry evidence, make sure beyond peradventure whether these of all quarries, situated at the first outcrop of argillite on the right river bank above Trenton, were the work of the supposed Drift Man or of the modern Indian.

Nine days' digging in eleven pits and two large shafts, measuring about 2,148 and 787 cubic feet respectively, showed us that the ancient disturbance reached from Gaddis Run about 299 feet up the steep ravine side, that the digging had been done by pulling out loose weathered masses by hand till the solid ledge was reached at a depth at the main entrance of seven feet, that the rock had not been attacked with fire, that large masses had often been pecked on their sides to split with the grain, and that — to guess roughly — about 14,070 cubic feet of stone had been overturned and worked.

We found 174 quartzite pebble hammer stones, four fire sites, numerous bits of scattered charcoal, three pitted hammers, 312 "turtle-backs," — if we may use the expressive word, — and besides the chips not another culture-telling trace of the workman.

Once more we had the trouble of imagining a quarryman who went home to meals, who never slept at the quarry, and, having done his work, hurried away too fast to leave any token of himself in this case except three pitted hammer stones.

Three out of 174 is too small a proportion to allow us to think that these familiar pitted tools were used at the quarry. They must have been brought there after having served their purpose elsewhere. If, as Mr. McGuire argues, they are the stone-carving implements *par excellence* of the man of the stone age in all time, then they infer neolithic culture wherever found, and here link the quarryman with the Indian, in whose village sites they are only less abundant than the bruised unpitted pebbles themselves.

As to the "turtle-backs," the question at once arose, Were they like the Trenton specimens? and the answer was fairly, I think, No. Much study and handling of them shows a broader skill and greater knowledge of the grain. The quarry specimens are thinner and struck out with far fewer blows; often they show four, and sometimes only two, facets to a side, averaging about six, while the Trenton specimens average about eight.

Had they been the object of the work, had they been finished implements, meant to be used by Drift Men or Indians, they would not have been abandoned to the extent of about one to every bushel of chips.

Made by blows with the pebble hammers, it was again plain that they were "wasters," cast aside in the process of making some desired form of stone.

But as it was impossible to suppose that all trace of the form aimed at had disappeared from the refuse, as no trace of a thinned blade appeared in all our digging, and as all our blade evidence consisted of "turtle-backs" and nothing else, we believed that a few of these lanceolate forms typified the blade wanted at the quarry.

There were, therefore, two questions to ask at this point:—

(1) Which of the "turtle-backs" were the good ones, that would have been carried away if they had not been unintentionally lost or broken?

(2) What were the "turtle-backs" wanted for?

Both questions were inferably answered later in an examination of two Indian flaking sites on the river shore, about 890 yards and half a mile away respectively. At these the blade finisher who had left no trace of himself at the quarries, had dropped polished celts, potsherds, arrowheads, and jasper chips to prove that he was an Indian, and we had the full story of the quarry and something more.

The full story of the quarry, because several argillite boulders and fragments that strewed the river shore had been partly worked, while fifteen "turtle-backs" of various sizes and shapes, together with chips like the quarry chips and quartzite-pebble hammers, lay scattered among them.

Something more, because among these coarse chips lay other fine knife-like flakes at once distinguishable from the quarry refuse, with two finished cache blades and thirty-one well thinned ends and points, positive enough proof that here the "cache" blades of argillite so common at all the Delaware village sites had been made by the geologically modern Indian, and that the "turtle-back" was a step, generally a false one, in the process.

When it appeared furthermore that we could connect these blade finishing sites with the quarries and that some "turtle-backs" had been carried down from the latter to the former places to be thinned, the significance of the ancient pits with their refuse was explained.

It made little difference that fourteen of these particular river side "turtle-backs" had certainly been made on the spot, and not at the quarry. The fact that they were "turtle-backs" used by the Indian blade-maker went far to explain their relatives of the quarry, but, when soon after we found one specimen and two points that seemed to have all the characteristics of quarry work, the inference was complete. The "turtle-backs" of the quarry had been made by Indians for final flaking into "cache" blades. But to ask how brought us to the other question, which particular ones would do, and were therefore not rejects, but good enough to take away; a question which involved a study of the grain of the stone and the method of chipping, and resulted in my finally choosing fifteen of the quarry specimens, generally points, which, with the three found at the flaking site, possessing thinness, lack of hump, and opposed surface planes as nearly as possible level with the grain, fulfilled to my mind the conditions of the "turtle-back" that would flake down by pressure or percussion.

So then the quarries had nothing to do with Drift Men, but were the work of Indians.

But one more question remained, When did the Indians work them?

The appearance of the workings argued against great age, but careful examination of the Indian village site that surrounded the riverside workshops above mentioned threw much light on the subject.

When we had dug two trenches 56 feet long by 3 feet wide by 6 to 11 feet deep, and 42 feet long by 3 feet wide by 7 to 9 feet deep, respectively, across the whole village site, and trowelled smooth the walls of sand to expose the clean strata, we saw that there were two layers of ancient habitation, an upper or surface 1½ to 2½ feet thick, and a lower of equal thickness 4½ to 5 feet down, separated by clean undisturbed stratified sand. Man had therefore lived below, and, after being overwhelmed by water and sand, had lived again above.

After making this important discovery, we carefully measured the depth of each specimen (above or below the water lines), and marked its association to learn what we might of the two periods witnessed by the sand. The main points were: —

(1) That, counting all the specimens, argillite was in the proportion of about 12 to 1 to all other artificial objects (excluding fire-fractured stones) in the lower layer, and of about 6 to 1 in the upper.

(2) That, against 58 jasper objects in the upper, there were only 7 small chips and 1 worked piece in the lower.

(3) That, while several triangular arrowheads were found above, none were found below, 13 of the 15 lower small blades being of the long narrow "fish-spear" type.

(4) That, while white man's glazed pottery was abundant above, none was found below, and that, against two potsherds of the coarsest Algonkin type below, there were numerous specimens representing four differently made varieties above, — some finely decorated, and some of the light type classified as South Appalachian by Mr. Holmes.

(5) That, of the dozen or more variously shaped "turtle-backs" found in each layer together with large worked masses of the material, all had doubtless been made on the spot from river stone, and not at the quarries, and all could be fairly said to resemble the Trenton specimens.

(6) That, with the possible exception of one fragment, no "cache" blades of the broad thin pattern or thin flakes were found below, while two perfect thin blades and thirteen ends and points lay scattered with hundreds of the flakes above, indicating the very important fact that the highly skilled art of sending off long narrow flakes and fashioning broad thin blades, a process evidently distinct from arrowhead making and as yet unexplained, belonged to the upper layer and the latest time of Indian occupancy. Granted that the comparative use of argillite was far greater in the older layer, granted the gradual introduction of jasper, still, to realize that the numbers of argillite blades of this thin class found belong to the later epoch, the epoch of white contact, is alone enough to prove a considerable use of argillite to the last.

In summing up this evidence, we realized that there was no ground for attributing great age to the flood that destroyed and buried the lower village, since the surface of the bank was only fourteen feet six inches above medium water

mark, and had been entirely covered by the freshet of 1841. Still, the difference in the remains from the two layers indicated difference in culture. We were shown an earlier time at that spot, before the coming of white men, when jasper was little used, when pottery had not reached its later perfection, when the thin fish-spear type of blade was common and the triangular arrowhead unused, when the important art of flaking large thin blades was undeveloped, and when all the blade material of argillite was gathered from the river, as some of it continued to be to the final discontinuance of the stone. Evidently the working of argillite and the manufacture of "turtle-backs" had continued for a time along the river side before the art of flaking broad thin blades was developed, and before the following up of the material by beach and stream had led the blade-maker to the discovery and systematic working of the quarries. We were made to realize that there were "turtle-backs" and "turtle-backs," "quarry rejects" and other "rejects," and while we wait for further proof to account for the presence of these famous objects in processes of blade-making, suspected, yet unknown, it is interesting to have sorted out two lots of them at least on the Delaware, those of the quarry and those of the river side, and to have seen clearly that the latter, and not the former, may be said to resemble the Trenton specimens.

While we know not yet what some of these river side forms may mean, the "turtle-backs" of the quarry forming a class by themselves must be connected, if we have properly judged their shape and the three specimens found at the river workshop, with the highly skilled process of blade flaking exemplified there and at the companion site at the village.

They belong to these two thin blade factories, and the blade factories belong, if we are right, to the later of the two epochs discovered at the village site of Lower Black's Eddy; in other words, to the final and highest period of culture there reached about the time of the coming of white men.

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Another ancient source of jasper blade material east of the Middle Alleghanies. By H. C. Mercer, Doylestown, Pa.

[abstract.]

I wish to call attention to the fact that the ancient jasper quarries discovered last summer in the Lehigh Hills were not the only sources of supply for aboriginal blade material east of the Pennsylvanian and Virginian Alleghanies. Because here are jasper pebbles water rolled and worked by Indians, found by me in the summer of 1892 on the beach of the Great Egg Harbor River in Southeastern New Jersey, and in the fall of 1891 on the Eastern Chesapeake beach at the mouth of the Choptauk River, Dorchester County, Maryland.

In both cases Indian village sites were close to the beaches on which the pebbles lay, and in both cases the Indians had worked the pebbles, because this partly worked piece showing a pebble surface was found among the flakes at the Great Egg Harbor village, and these at the Choptauk site. At both sites the numerous scattered flakes matched the beach pebbles in color and texture.

If these pebbles, which at either spot did not average above one and a half inches in diameter, were washed from the hill outcrops, the means of their transportation is yet to be accounted for. But whatever their geological origin, they prove that the tide-water Indians at the places named did not have to resort for jasper blade material to the hill quarries, respectively ninety and one hundred and forty miles away.

I found similar worked pebbles of black jasper or chert at the Delaware (west shore) Indian village sites of Pidcock's Creek, Taylorsville, and Lower Black's Eddy, and these chipped evidently down to the time of white coming.

Just as Mr. R. Brough Smith says that some of the native Australians near Gippsland, Victoria, made hatchets from "greenstone" river pebbles, while others near Kilmore, despising the pebbles, resorted to a quarry on the Mount Hope Range, so some of these people seem to have continued to get blade material from the river bed while others worked the quarries.

As this is true of the black jasper or chert pebbles which lie by the million on the Delaware beaches near Lower Black's Eddy, and so could hardly have been avoided by a blade-making people, it is probably also true (though to a less extent) in the case of red and yellow jasper; for I found a yellow jasper pebble at the Pidcock's Creek site, another near the Tohicon's mouth, and another in Gaddis Run.

None were chipped, though the Gaddis Run specimen seems to have been fractured by fire. But the presence of these pebbles is significant, and proves with the other evidence noted that the discovery of small jasper chips at a Delaware village site does not infer a knowledge of the quarries by its inhabitants. No more does the similar occurrence of argillite chips prove the status of culture evidenced by the argillite quarries recently discovered; for the Delaware beaches near Point Pleasant are littered with argillite blocks ready to the chipper's hand, and there is abundant evidence at many sites that the Indian continued to the last, and while the quarries were in full operation, to work this latter blade material from the rough close to the shore.

Investigation strengthens the evidence that on the Delaware some time elapsed before the river people discovered the quarries; that argillite, chert, and jasper itself, first found and used in the bed of the river and its tributary streams, were traced by degrees along what might be called a pebble-strewn pathway of blade material to the recondite outcrops of the native rock, where the valued material was at length systematically quarried.

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ANTHROPOLOGICAL WORK AT THE UNIVERSITY OF MICHIGAN. By HARLAN I. SMITH, Saginaw E. S., Michigan.

[ABSTRACT.]

THE paper states that, beginning with the second semester of the college year 1891–92, a course in Museum work in American Archæology was offered at the University of Michigan, and an Anthropological Laboratory was established, and that specimens previously stored have been placed on exhibition in proper cases, or taken to the laboratory to be worked upon.

THE INSTINCTIVE INTEREST OF CHILDREN IN BEAR AND WOLF STORIES. By Prof. WM. H. BREWER, New Haven, Conn.

[ABSTRACT.]

THE children of European races take more interest in bear and wolf stories than in stories relating to any other wild animals. Their interest in bears is greater than that in wolves, and in the plays of children bears have a much more conspicuous part. There is a sort of fascination in everything relating to these animals that attracts the child's attention from a very early age, and "Tell me a bear story" is a common request long before it learns to read.

I had something to do with the exploration of the mountains of the Pacific Slope and of the Rocky Mountains before railroads and settlements had removed the frontier beyond which wild animals and Indians roamed free. I have often been amused since, when talking with friends of the incidents and adventures of those explorations, to have some child who chanced to be listening to the conversation burst out impatiently with the all-important question uppermost in his mind, "Did you see a bear?" However ignorant of the geographical distribution of animals they may have been, they never asked me if I had met lions, or tigers, or panthers, or buffalo,—it was a "bear," or sometimes a timorous question as to whether there were any wolves.

I have experimented on my own and other people's children regarding their special interest in these two animals, and have pursued inquiries among my friends. I could never excite much interest over any story about lions or tigers, or other rapacious animals I had read about, nor about panthers I had met, but a bear story would always interest, no matter if it was a second-hand story and not told as a personal adventure. Tell a lion story and the child soon wants another, but a bear story never grows old from mere repetition, until the child grows old and wants new amusement.

Let me illustrate what I mean. When camped near Carmelo Bay on the Pacific coast, bay whaling was pursued in the vicinity. A whale that was mortally wounded but not captured was cast up among the rocks near our camp, and a large grizzly bear came down to the shore on moonlight nights to feed upon the carcass. A few weeks later, while visiting in the family of a friend in town, I mentioned this while talking about the whaling industry. A year or more later I chanced again to visit this family, and immediately a little four year old boy climbed upon my lap and began to question me about the bear. I did not understand what bear he was asking about until the mother explained, half laughingly, "Ah, Professor, you little know what a task you put upon me by your visit last year, when you told about a grizzly bear eating a dead whale; every blessed night since I have had to tell that story over again, and woe to me if I left out the slightest part."

This is perfectly typical of the special interest these animals awaken, and I need not illustrate my meaning further. Most mothers are familiar with some phase of it, and I think that you will all concede the general fact of such interest. Try to imagine the story of Little Red Riding Hood with the animal of the story a panther, or a leopard, or a hyena. And this special interest never dies out entirely. Who of us remembers so distinctly the Bible story of Samson

and the lion as he does that of the bears who devoured the naughty children who made fun of the bald-headed prophet. I am told that in India there is no special interest manifested by children for bear stories, but that there is in those relating to tigers.

Now, why this special interest by our children in these two animals?

There are two explanations. The first is, that it is entirely a matter of education with each child; that the conservative traditions of children have preserved more stories about bears and wolves, that parents and nurses talk more about them, and that these animals have a larger place in the literature for children; hence the special interest.

The other explanation concedes that education is a factor, but that the interest is intensified by instinctive suggestion. I am convinced that this is the true explanation. No other theory so well explains all the phenomena. And if instinctive, then its origin is a matter of much scientific interest, for the origin of the instincts is now a mooted question among naturalists.

Until lately all evolutionists have believed that instinct originated in part as the inheritance of an acquired character; that it was in part the inheritance of experience, inherited memory, inherited habit, inherited education; that experience, habit, and education stamped theoretically upon the brain substance, and that this stamp is transmitted by heredity to some degree, and that, if similar impressions are made on the brains of many successive generations, it becomes permanent and manifests itself in special instincts. This theory is now denied emphatically and *in toto* by the naturalists of the Weissmannian school.

My own belief is that the special interest our children show toward these two animals is instinctive, and that it is of the nature of an inherited memory, vague to be sure, yet strong enough to give a bend to the natural inclinations. This interest of children in these animals is general and very widespread. It exists in many countries, among peoples speaking many languages and differing greatly in religion, in customs, and in traditions. I cannot understand how it could possibly have assumed such a general character, under such varied conditions, by the separate education of each individual child. Nor can I understand how this instinct (if it is an instinct) grew into its present shape by the natural selection of adventitious variation of some previous instinct. If it is an inherited memory, then it is easy to explain. These two wild animals are the ones which have been and still are the most destructive to human life (and particularly to children) in our latitude and climate. Abundant statistics show that there is still a considerable destruction of children in various European countries by bears and wolves, and this must have been vastly greater in the past ages.

Several of the larger breeds of dogs were originally evolved as wolf dogs for the protection of sheep and children. This was the origin not only of the wolf-hound proper, but of the mastiff, particularly the Spanish mastiff, and even of the St. Bernard. Government bounties are still offered for the destruction of wolves in several European countries.

The fear inspired by these animals during the long ages of the childhood of our civilization, and the education of the many successive generations of our ancestors in this fear, descends to us as an inherited memory, or in other words

an instinct. While not strong, it is of sufficient force to create that kind of fascination which stories of bears and wolves have in childhood, before the instincts are covered up and obscured by intellectual education. The great shaggy bear appeals more strongly to the imagination of children, hence its superior value to play "boo" with.

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THE DELICACY OF THE SENSE OF TASTE AMONG INDIANS. By E. H. S. BAILEY, Lawrence, Kan.

[ABSTRACT.]

THIS paper is a continuation of investigations carried on in the same general way by the author for several years. The experimenters are requested to classify certain liquids of known strength and composition. The substances used in these tests are quinine sulphate, sodium chloride, sodium bicarbonate, sulphuric acid, and cane sugar. These represent the five tastes bitter, salt, alkali, sour, and sweet. Tests were made upon some of the pupils at Haskell Institute, a government Indian school. From these experiments, made both with males and with females, it was found that in all cases, as far as observed, the delicacy of the sense of taste was below that of the average of white students. Further, it was noticed that in most cases the females possessed a more delicate sense of taste than the males. The only possible exception to this rule was in the case of common salt.

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ON SONGS OF SEQUENCE OF THE NAVAJOS. By Dr. WASHINGTON MATTHEWS, U. S. Army.

[ABSTRACT.]

THE great majority of the very numerous songs of the Navajo rites are divided into groups. During the progress of the rites, these groups follow one another in an established order. For this reason the author calls songs of this character "songs of sequence."

In a previous paper, "The Mountain Chant," published in the Fifth Annual Report of the Bureau of Ethnology, the author has mentioned some of the more important rules concerning songs of sequence. The object of the present paper is only to show how the order of the songs in each set is remembered.

The key to this order is a special myth for each set. As an illustration, the author repeated the myth of the "Songs in the Farm of the House God," and either recited or described each song in the place where it was introduced in the myth. There are thirty songs in this set.

"In some instances the myth is the more important part of the work, and we are impressed with the idea that the myth-maker composed his story first, and introduced his songs afterwards as embellishments; but in more cases the myth is a trifling element, and seems devised merely as an aid to memory or as a means of explaining or giving interest to the songs."

At the conclusion of the paper, some of the songs which the author had recorded on the Edison phonograph were repeated to the audience from that instrument.

REMARKS ON THE MEXICAN CALENDAR SYSTEM. By Dr. D. G. BRINTON, Media, Pa.

[ABSTRACT.]

THE arithmetical basis of the Mexican calendar is a combination of the figures 13 and 20. It has been hitherto undecided to what astronomical events, if to any, these figures bear reference. Some believe they are connected with the motions of Venus, others with those of the moon. The calendar in its first form had no reference to the solar year. Its adaptation as a year count came later, and with differences of detail in different nations. Much obscurity rests over many of these points. Originally this calendar was employed exclusively for purposes of divination and religion. The meaning of its symbols is obscured by time, but they can still be seen in general outline to represent the life of man and the agencies which influence it.

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THE PREHISTORIC MAN OF MEXICO. By ALPHONSUS S. HERRERA, Mexico.

[ABSTRACT.]

DESCRIPTIONS and photographs of a lower maxillary, human, from the post tertiary of the valley of Mexico; of a worked bone from the same horizon; of pottery, and of bones from strata under the lava; and of stone implements found adjacent to bones of extinct animals. The author concludes that the evidence proves that man lived in Mexico in early post-tertiary times, and had acquired a considerable degree of civilization.

[This paper will be printed in the Memoirs de la Sociedad Cientifica Antonio Alzate, Mexico.]

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OBSERVATIONS ON THE USE OF ARGILLITE BY PREHISTORIC PEOPLE IN THE DELAWARE VALLEY. By ERNEST VOLK, Trenton, N. J.

[ABSTRACT.]

IN my recent explorations of old village sites in the Delaware Valley, during the summer of 1891, under the direction of Professor Putnam, Chief of Department of Ethnology of the World's Columbian Exposition, I made some observations to which I wish to call the attention of members of this Section.

In exploring a village site near Trenton, N. J., situated on the terrace which borders the city on the southeast, I made extensive excavations. The soil in this place consists of nine inches of black or subsoil, tilled land, overlying an undisturbed sandy loam composed of quartz sand colored by iron and mixed with a yellow soil the sediment of muddy water, the whole having a light yellow color. Three feet below the surface is a somewhat uneven stratum of red clay mixed with sand. The top or subsoil, for the depth of six or seven inches, is filled with the relics of an Indian village site, consisting of chips and flakes of various colored jasper, the yellow predominating, black chert, quartz and quartzite flakes, and chips of argillite, with whole and broken implements of all these different materials. The finding of thin flakes of argillite among the

others is not surprising, but the fact that at the bottom of the disturbed soil, on the junction with the yellow sandy loam nine inches below the surface, the argillite flakes were larger and not associated with any other material except broken quartzite pebbles, is of importance as suggesting the earlier use of argillite. I have found a large number of broken quartzite pebbles, although of smaller size, among the other relics nearer to and upon the surface, and they were undoubtedly used as hammer-stones. The rule, "The coarser the object the coarser the tool," seems to have prevailed here.

An adjoining village site on the west, explored the same year, showed precisely the same features; not a flake or particle of any other material except argillite could be found at the junction of the disturbed soil with the undisturbed.

The following year, 1892, I fortunately struck a much older village site, and the observations made there removed all doubt from my mind as to the fact that argillite was first used in the Delaware Valley long before any other material was used, and that its use was never abandoned up to the time of European contact.

The features of the previously explored village sites, in regard to the finding of argillite alone below the surface soil, were not only repeated here, at a much greater depth and far away from the others, but the fact was established that this place was inhabited long before those on the highland or terrace, and that the introduction of jasper came much later than in the Highland village sites. This ancient village site is situated in the so-called Lowland or meadow three miles southeast of Trenton, N. J., on an extensive flat or marsh that follows the Delaware River for six miles on its eastern shore.[1] This land is from forty to sixty feet lower than the village sites explored the previous year, on the terrace. This old village site is on the eastern shore of an old stream, short, but picturesque, winding its way through the marshes toward the river. The stream has no name now, but had one in olden times. Thomas Campanius, who, in 1654, wrote the history of New Sweden, as New Jersey was then called, says about this stream, "The Poeatguissings Creek is provided with everything that man can desire." This name as it is given has suffered much mutilation (see Dr. Abbott's book "Upland and Meadow") and was formerly Achpoachgussiuk, which according to Dr. D. G. Brinton is an Indian name, meaning the place of the corn bread baking,—"Ach-poan," corn bread, and "Ach-gussen," to roast or cook. The early settlers soon made it Poeatguissings Creek, as it was called for a long time. The tribe found near it by the Swedish writer was called the Wantees or Mantees Indians, who claimed the land by right of possession. This old stream is now much changed. Canal and railroads have cut off its free access to the Delaware, and artificial dams have turned the once lively current into a sluggish stream, raised its water level, and deposited over the clay bed a vegetable mould two or three feet in depth. The little fertile plain, now about seven feet above the summer level of the creek, has not been idle since the departure of the people who had chosen it as a home, and an accumulation of 15 to 24 inches of soil is the result of the vegetable growth that has gone to decay. This little plain I explored during the summer of 1892 by dividing it into eleven

[1] Andrew K. Rowan, on whose land this village site is situated, kindly gave me permission to explore the place.

trenches running from the creek eastward. The total area explored makes a length of 450 feet parallel with the creek north and south, and 30 to 90 feet east and west. In this place were graves, pits, fire pits, hearths, post holes, caches, ash beds, workshops, and small heaps of pottery. After removing the 15 to 24 inches of soil, we found the original surface of habitation, a stratum of black soil, in many places two feet deep, running downward and becoming lighter in color as it reaches the undisturbed sandy loam, which varies in depth from one to two feet, when it strikes the plastic clay. This clay underlies the whole plain and forms the bed of the creek. The graves were six in number, and scattered, but all were near the little bluff. Three of them contained each a single skeleton, one of which was without a skull. Two contained two skeletons each, and in one of these the two skeletons lay on the same level, while in the other one skeleton was on top of the other. All these skeletons were buried with the knees drawn up toward the body which lay on the side. The south grave contained five skeletons, all extended at full length on their backs. The soil in these graves varied. Numbers four and six had black soil or vegetable mould; the others resembled those of the Highland, the soil being reddish with particles of charcoal and small fragments of pottery mixed through it. There were evidences of fire in connection with the burials. All the skeletons were badly decomposed. Grave four is particularly interesting, as here we find proof of a long occupation of this place by prehistoric people.

The soil surrounding this grave is of a light yellow color down to the clay, which is here five and a half feet below the surface. For a space of twelve feet in a half-circle, and down to the clay, it is filled with thin argillite chips loosely strewn in the soil. Into this a pit has been dug, two feet in diameter and the same in depth. The pit contains decayed fragments of animal bones and small fragments of pottery. The soil in this pit is darker than the outside soil. The grave was dug into this, taking off a part of the pit and workshop. The filling of the grave is a rich soil, almost black. Thus we have here the evidence of three distinct periods of occupation: the workshop is probably the oldest, and its soil is a deposit probably made by muddy water; then the pit, which corresponds with the pits on the northern half of the village site; and lastly, the grave, filled in with leaf mould and rich soil, and apparently belonging to the most recent period. The skeleton does not differ from the others in state of preservation and is much decomposed. The leaf mould overlying the present surface in the forest is not over three inches thick and it would require scraping fifty or more feet to gather enough to fill the grave.

During the exploration of this village site there were found in the eleven trenches 190 pits of various sizes and depths, which furnished very interesting evidences of a long continued occupancy of this place. The majority of these pits started at the beginning or middle of the black soil, 15 or 20 inches from the present surface, and ran downward, having apparently been dug when the black soil was accumulating. Two pits, apparently the oldest, started just below the black soil, 33 and 35 inches from the present surface, and in these pits nothing was found but rude quartzite boulders, three or four inches in diameter, broken and mixed with small particles of charcoal. The soil is yellow sand like that at the mouths of the pits. There were other pits at varying depths from six to ten inches below the present surface running down through the

black soil, and these were evidently dug at a later time, and are mostly at the northern end of the village site. The largest pits in diameter and running down to the greatest depth from the surface are found in the southern half of the village, and the specimens found in the large pits are generally rude pottery, argillite flakes, thin and rude, and also chipped boulders and broken rude implements of the same, large bones of animals, and pieces of mica. The medium-sized pits on the northern end of the village contained chiefly thin, nicely decorated pottery, bones of smaller animals, a few fragments of rude pottery and mica, chips and flakes of argillite, and occasionally jasper and chert. Argillite was found in all the pits, and always predominated, but not so jasper, except in one case, where the jasper chips were the most numerous. It is evident that the medium-sized pits, starting from points nearest to the surface, are the most recent. Three of these pits correspond with those on the Highland, as they contain precisely the same kind of specimens, bird bones, deer bones, bones of small game, and bone implements, all well preserved, fin bones of a small fish, and turtle shells. The soil also corresponds with the Lalor Field pits on the Highland, and is of decayed organic matter chiefly.

In general digging at the northern end of the village there was found near the surface a red brittle pottery of very rude make, lightly baked, and having much sand mixed with the clay. This pottery seems to be of comparatively recent date. Numerous mortar stones were found in these pits, but all are rude and have no cavities worked in them except where worn away in the centre by use. Equally rude are the pestles, simple water-worn stones. A pestle and apparently a celt of argillite, from one of these pits, are of the rudest character ever found in this locality. The argillite implements are as a rule rude, and those of a knife pattern differ from others found in this locality. Many unfinished argillite implements were found in the deep pits, but always at the bottom.

A layer of large partly broken quartzite pebbles, larger than the common run of these stones, was found in the shape of a hearth in a coarse yellow sand, some depth below the top of the black layer in the sandy deposit, and particles of charcoal were mixed with the stones. The black layer being undisturbed, this hearth must have been built and used long before the black soil was deposited. The pottery found in the black soil is of medium-sized vessels, but in small fragments, except when found in pits. One peculiarity is a pit found with a level bottom of burnt clay. Another is a shallow pit in trench 9 which contained fragments of pottery such as generally found on the surface of the Highland.

Besides these pits we found here hearths of the same variation in depth as the pits, one reaching below the black soil, many on a level with it, and several above. The ash beds were only three in number, but all are in the black soil, never below it, beginning with the top or latest portion of the black layer. They were distinguished by their intense charcoal-black color, still blacker than the black soil.

The charcoal pits, or, as I have designated them, the fire pits, are a group of small pits scattered through the trenches; they differ much in depth and diameter from the others, being much smaller and nearly all of the same depth and diameter. They are never more than ten to twelve inches below the surface, and in many cases nearer to the surface. They contain nothing but charcoal, and

belong to the most recent occupation of this locality by prehistoric people. In some of the hearths mentioned above were found broken granite celts: these were hardly ten inches below the present surface, and at the northern end of the village. No other polished implements have been found in this village site, neither have we met with any trace of metal. Everything is rude and plain.

We must now consider the various specimens found mostly in general digging. In trench 1, we found a little heap of bones 10 inches under ground, and another 12 inches; two caches 11 inches below the surface, one containing a number of unfinished implements of argillite, and the other, not deeper, containing eight beautiful large argillites, finished blades; in trench 5, a little heap of rude thick pottery marked by a coarse fabric, and a little heap of antler points at 8 inches; in trench 7, a little heap of unfinished argillites a trifle smaller than those found in Wright's Field on the Highland, the previous spring. 14 inches deep; in trench 8, three finished argillite implements at a depth of 10 inches and a mortar stone at 12 inches; in trench 9, we found four of these little heaps of pottery, the red brittle kind that does not stand handling. Trench 10 contained several of these heaps of pottery, all near the surface and apparently of recent date. Among these things so near the surface we found no chips nor flakes of any kind, but a foot or so deeper we struck many argillite chips accompanied by thin chips of jasper, chert, and a few chips of white quartz. Reaching the black soil we found the argillite and jasper chips associated, but the argillite predominating, and of large and small size, with finished implements of both. When we reached the lower deposit of black soil the jasper and chert became scarce and finally entirely disappeared, and the argillite alone remained, now in coarser flakes and rude boulders, until we reached the bottom of the black streak, now faded out, and on the edge of the undisturbed and sometimes two or three inches into the yellow deposit we picked out a rude chipped argillite boulder and the accompanying large broken quartzite pebble. This scattered layer of broken argillite runs at this depth through the entire village site. Not once in all the time spent in exploring this village site did we find a jasper flake, chip, or chipped pebble in this lowest layer.

Having saved all the specimens of stone found in this village site except the broken quartzite pebbles with which the ground a foot below the surface is almost filled, we now have the material for reading the whole story. When we look at the unfinished specimens we have found in the caches we should naturally think that these were chipped where the material was plentiful and brought here to be finished at leisure; and the several pits we find with their thin argillite chips and chert hammer-stones, as round as a ball, strengthen this idea. But when we examine the lower finds of argillite on the undisturbed soil we see at once that the chipping here was much simpler, a ruder implement answering all the needs. Having a complete collection from the same place, as I said before, makes these facts plain; and when we take into consideration the facts stated, and see these forms on the bottom or first surface of settlement at this place, we cannot help coming to the conclusion that a continuous improvement and a gradual introduction of another material than argillite took place. We see that, as time went on, the large game whose bones were found in the pits became scarce, bones of smaller animals being found in the pits of medium size nearest the surface, and bird bones making their appearance. This necessitated an im-

proved implement, and consequently a new material was introduced and a finer form was given to the argillite. The rude vessels made room for less capacious ones, and the taste for ornamentation began to show itself; only in straight lines, it is true, but fancifully arranged. The polishing began later, and in general digging on the northern side, six inches from the surface, there was found a broken gorget with a polish. Two broken celts of granite having a polished cutting edge were found in a hearth near the surface, but we were not successful in finding any chips or flakes of that material in the village; consequently they must have come from some other place.

From the exploration of this village site it appears that there was a continuous occupation of the place since the first rude argillite fragment had been deposited there at a depth from the present surface of three to four feet. The collection made here is so complete that it cannot fail to tell the history of the people who lived here at different periods of time.

The whole collection from this village site, as well as two others from the Highland or terrace in the nighborhood of the former, are on exhibition in the Department of Ethnology at the World's Columbian Exposition in Chicago.

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PRIMITIVE WOMAN AS POET. By DR. A. F. CHAMBERLAIN, Clark University, Worcester, Mass.

[ABSTRACT.]

OF poetry it might well be said, *Dux fœmina facti*, for song is as natural to the mother beside the cradle of her tender offspring as is speech to man himself. The old German poet Fischer rightly said:

> "Wo Honig ist, da samlen sich die Fliegen,
> Wo Kinder sind, da singt man um die Wiegen."

Lullabies are known in every land, and "the folk-poetry of all peoples is rich in songs, whose text and whose melodies the tender mother has herself imagined and composed." (Ploss.) But not cradle-songs alone are the product of the genius of woman. As in modern, so in primitive times, maidens inspired by love have vented their feelings in song. We find such poetesses amongst the Arabs and Bedouins of the desert, in Polynesia and Australia, Madagascar, etc. Women *improvvisatori* are known amongst the American Indians, in Bambarra and Loango (Africa), etc. The share of woman in the transmission of song and story from generation to generation is very great; indeed, amongst some of the tribes of Guiana the bards of pre-classic Europe are represented by the old women, the conservators, modifiers, and transmitters of legend and myth.

Among the Bedouins, as elsewhere, women and girls have special songs. These are never imparted to the men, and it is very difficult, usually impossible, for a traveller to obtain texts.

IS THE POLYSYNTHESIS OF DUPONCEAU CHARACTERISTIC OF AMERICAN INDIAN LANGUAGES? By J. N. B. HEWITT, Bureau of Ethnology, Washington, D C.

[ABSTRACT.]

POLYSYNTHESIS is a hypothetic scheme propounded by Duponceau in an attempt to systemize the varied morphologic processes of the known American Indian tongues. He was led to believe, but on untenable grounds, that he had discovered the distinctive features of a conjectured ground plan common to all these tongues. The etymology of the word "polysynthesis" as found in the dictionaries furnishes no clue to the character of the system. Its use in American Indian linguistics is a bar to the just comprehension of the morphologic phenomena of these tongues, because no two writers agree in giving it the same definite meaning. Duponceau used it as a mere designation or label, without giving it any other importance. Not satisfied with this explanation, some have attempted to read into it a meaning which its coiner never associated with it; these have attempted to rehabilitate Duponceau's polysynthetic system, by efforts to give their own definitions of what they believed he had in mind. The facts of grammar, however, found in these diverse languages, do not confirm the positions on which the system was based. It assumed the use of fragments of words and phrases, — "the most significant sounds or syllables of each simple word," — and also the employment of an alleged syntactic process of "interweaving together" these and other elements (parts of speech) into words. No Indian language that has been critically analyzed uses either the material resulting from the mutilation or decomposition of words and phrases, or the process of intercalation of particles and words into the substance of another word, and therefore no such Indian tongue is polysynthetic.

Dr. Francis Lieber coined the word holophrasis to supplant the names agglutination and polysynthesis, for he believed that they were misnomers of a system wherein synthesis antedated analysis. "For," says he, "these names indicate that that which has been separated is put together, as if man began with analysis, while in fact he ends with it." The science of language shows us that this is a palpable confusion of the so called analytic form of expression with mental analysis. The syntheses which are taken by those who hold to this doctrine as original forms have not the appearance of things thrown confusedly together. Ideas and their proposed or intended relations find in them explicit designation, which presupposes in those who use them skill in the use of language to enable them thus definitely to express them. In other words, these derivative forms and word-sentences represent the orderly results of antecedent mental analysis.

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BURIED DEPOSITS OF HORNSTONE DISKS. By Dr. J. F. SNYDER, Virginia, Cass Co., Ill.

[ABSTRACT.]

FROM the recent researches of Mr. Warren K. Moorehead in Ohio, made for the Department of Ethnology, World's Columbian Exposition, under the direction of Prof. F. W. Putnam, Chief of the Department, we learn that in a certain

ancient mound, situated near the North Fork of Paint Creek, a tributary of the Scioto River, in Ross County, over 7,000 ovoid disks of dark-colored flint, known as "hornstone," were found buried at its base. Messrs. Squier and Davis, who, in 1847, were the first explorers to disturb the integrity of construction of this mound, describe it as having in its composition two strata of sand, separated from each other by a foot or more of intervening earth, and continuous throughout the structure to the ground; and that those mysterious seams of sand had never, to that time, been broken.[1] Mr. Moorehead, who subsequently removed the entire mound, found the sand strata intact, excepting in the centre where the previous exploring work had been done. He adds the fact that this mound, as well as all the others in that group, had "been constructed on a hard burnt floor"; and that on this floor prepared by fire the flints were found "lying in little pockets or bunches of twelve to fifteen each with layers of sand between each mass."[2]

In the Annual Report of the Smithsonian Institution for the year 1876 I published an account of two deposits of similar flints found buried on the banks of the Illinois River. The first of these, discovered at the village of Frederick, in Schuyler County, in 1860, comprised about 3,500 flints; the other, found in the city of Beardstown, Cass County, on the opposite, or eastern, side of the river, four miles lower down, contained 1,580. The disks in both were buried in the ground five feet below the surface; and neither deposit was marked by mound or other discernible object or sign to denote their place of burial. In the deposit at Frederick the flints were brought to light by the banks of a small rivulet falling in after a freshet; but, unfortunately, no notice was taken of the arrangement of the disks in their burial. In making the deposit at Beardstown a pit shaped like the flints, with apex up stream, had been sunk near the river's edge, five feet in depth, down through the sand to the underlying drift clay (*loëss*). The flints were laid in this pit in five courses, separated from one another by layers of clay two inches in thickness; and in each course the disks were carefully placed with their pointed ends up stream and overlapping one another, as slates are arranged on a roof; and the pit was then filled with sand. In neither of these burials was there any indication of the action of fire, either above or below the flints. The deposit at Beardstown, which I closely examined throughout the process of its exhumation, presented every appearance of having remained there untouched for a great period of time. Many of the flints were heavily incrusted, and others firmly cemented together, with calcareo-ferruginous concretions, almost as hard as the stone itself, derived by percolation from the enclosing clay.

A third deposit of hornstone disks, about 1,000 in number, was found several years ago at the base of a mound in Union County, Illinois, near the Mississippi River, by Mr. Thomas M. Perrine, who published but a meagre account of his discovery. I am not positive as to the exact number of flints in this deposit; but many of them that I have examined were precisely similar to all the others heretofore mentioned. It is to be regretted that Mr. Perrine paid no attention to the structure of the mound, or to the disposition of the disks that it covered.

[1] Ancient Monuments of the Mississippi Valley, p. 158.

[2] Primitive Man in Ohio, W. K. Moorehead, 1892, pp. 186, 190.

All of the 12,000 disks taken from the Ohio mounds and the deposits in Schuyler, Cass, and Union Counties, Illinois, with the one exception stated by Mr. Moorehead,[1] are in every essential particular identical. Some in each lot are more neatly finished than others; a few are circular, and others are oblong and rounded at each end: but all were chipped to a cutting edge all around, and none had ever been used. The type pattern aimed to be followed in their manufacture, and to which the great majority of them conform, is the mulberry leaf, rounded at the base and more or less pointed at the apex. From their striking similarity of shape, uniformity of size, and sameness of material and method of making, and from the lithological identity of the few unbroken nodules of dark flint in each deposit, — evidently as specimens of the stone from which they were worked, — the conclusion is irresistible that they were all derived from the same locality, and were made and buried by the same people.

In the summer of 1890 I partially explored one of a group of very interesting mounds situated near La Grange, in Brown County, Illinois, on the west side of the Illinois River, thirteen miles below Beardstown. It was 35 feet high, 100 feet wide, and 180 feet in length. A pit fourteen feet square was sunk in its middle down to its base and then trenched out to one side, and tunnelled under in places, by which an approximate knowledge of its entire structure was gained. The series of mounds to which this one belongs, five in number, were built on the alluvial river bottom, of clay (*loëss*) brought from the bluff 100 yards to the west, and 300 yards from the Illinois River to the east, where its channel was at that time, now marked by a chain of sloughs and shallow lakes. The bed of the river at present is nearly a mile farther away to the east. The builders of this mound, the largest of the five, in commencing its construction, had first laid down an oval-shaped bed of clay on the sandy soil twenty feet in width, thirty feet in length, and two feet or more in depth with saucer-like depression of surface. On this clay platform fire had been maintained until it was burned hard for several inches below the surface, and its concave top was filled with ashes in which many fragments of charred human bones were observed. After the fire had been extinguished and the bed of ashes cooled there was laid upon it a mass of hornstone disks covering its whole extent a foot or more in thickness, and apparently placed as those were in the Paint Creek mound, as observed by Mr. Moorehead, in lots of fifteen or twenty, with sand between each lot as though to keep them isolated. Among the flints were two entire nodules of the raw material and a few others broken in pieces. From this deposit 6,107 chipped disks were taken out, without positive assurance that the store was yet entirely exhausted. The flints from this mound are, in the average, smaller and of ruder and inferior workmanship than those of either of the other deposits described, but the stone of which they are made is the same, and in outline they preserve, in the main, the same mullberry-leaf figure. This identity of material and type, and mode of burial, together with the same kidney-shaped nodules of dark flint with yellowish white *patina* envelopes, as found in all the other large deposits, point unerringly to a common origin of all and identity of ethnic impulses inspiring their fabrication.

[1] Primitive Man in Ohio, p. 189.

Upon this mass of flints had been spread a stratum of clay ten inches in thickness, and on this another glowing fire had been continued for some time in which were cremated many human bodies, or skeletons. With the numerous pieces of burnt human bones we found several large marine shells, sheets of mica, spool-shaped ornaments of copper and a half-crescent of hammered copper, bone awls, broken pottery, stone celts, flint arrow and spear heads, necklaces of *marginella* shells and of the large incisors of bears partially drilled for the insertion of stone settings, and other objects; but all so broken and calcined by the intense heat that only a few could be restored and preserved. The fire seems to have been extinguished by covering it with clay, and the whole pyre was then enclosed with huge logs that had long since disappeared, leaving only dark streaks of vegetable dust at the bottom of their empty casts in the dry clay. Finally, over all, the immense mass of clay had been carried from the bluffs and piled up to form the mound, that must have been forty feet high when first completed.

The quarries from which the hornstone nodules for making all these disks were obtained, and the location of the open-air workshops of the ancient artisans who broke them and chipped the broad fragments into prescribed forms, have not yet been discovered. No flint of this kind, in any quantity, is known within the limits of the State of Illinois, nor in any geological formation of Ohio. Consequently, this aggregate of 20,000 flints were conveyed perhaps several hundred miles after all the vast labor of their production.

In considering this class of American antiquities the inquiries suggested are: For what purpose were they made? And what motive could have prompted the stupendous labor of quarrying, cleaving, chipping, transporting, and burying such surprising numbers of them?

An apparently plausible hypothesis advanced in solution of these questions is, that the disks are but "roughed-out ingots" that had been *cached* in the ground for concealment and safety, and to preserve the better cleavage properties of the stone, to be withdrawn when needed, and reduced by further chipping to finished implements.

A little reflection, however, will raise serious doubts as to the correctness of this theory.

On the threshold of this discussion we must bear in mind the many small deposits of flint implements found in all parts of the Mississippi Valley, placed in the ground by nomadic people, without order or ceremony, for the very obvious purpose of concealment and safety until necessity again required their use. Some of these hidden articles are plainly unfinished blades of chert, or white flint, intended to be worked over when convenient. Many other deposits of this kind are of broad, flat, well finished implements of hornstone, very similar to the disks before described, but smaller, and always bearing marks of long continued and hard usage.[1] The number of flints in all these unquestioned temporary *caches* is usually limited, and in but few instances exceeds in weight what one person can easily carry. They were buried but a short distance below the surface, and at the time of hiding were probably enclosed in bark or other per-

[1] See Antiquities of Wisconsin, I. A. Lapham, 1853, p. 8; Smithsonian Annual Report for 1872, p. 402; Ibid. for 1879, p. 436.

ishable material. The *caches* of well-worn hornstone implements are found invariably near watercourses. The flints are of the same mulberry-leaf pattern as the disks in the large deposits, and, in my opinion, were simply canoe-making tools stored in the ground after the completion of a "dug-out" for safety until again wanted for similar use. My reason for this belief is that many years ago I saw Indians on Snake River, in Oregon, make stone implements of exactly the same form and proportions as these, of hornblende, or aphanite, which they employed in scraping down and removing the charred and loosened wood in the process of canoe-making by hollowing out the log with fire. When the tools became dulled from use they were rechipped, or sharpened; and this was repeated when necessary until in time the stone was materially reduced in dimensions.

The makers of gun-flints, a century ago, learned by experience that flint of all kinds chips most easily when newly quarried, or is kept damp and excluded from the drying action of the atmosphere, and therefore stored their raw material in tanks of water, or buried it in moist ground, until wanted for manufacturing. This knowledge, and the additional fact that none of the disks in the large deposits were ever used, are relied upon to sustain the idea that the disks were only "roughed-out ingots," and the deposits nothing more than temporary hiding places.

The artistic status of the disks may remain, in a measure, a matter of individual opinion and judgment. Different degrees of mechanical skill are evinced in their fashioning; some are quite rough, but the greater number are as nicely proportioned, and as fine specimens of the stone-chipping art, as can be seen in the best class of flint implements. A critical examination of them by any competent archæologist will, I think, convince him — as the inspection of thousands of them has satisfied me — that each flint is finished and complete for its intended purpose.

A moment's thought will dissipate the notion that their burial had any view to preserving the chipping qualities of the stone; for we know the position of the thousands of them in the Paint Creek and Brown County (Illinois) mounds was, until violated by recent scientific vandals, absolutely anhydrous. And, further, their receptacles there had been previously dried by fire, and their environments were intentionally so arranged as to render their contact with the slightest moisture almost impossible.

On the assumption that the great deposits of disks were only *cached*, as temporary expedients, what explanation can be offered for the singular care and ceremonious observance of their manner of hiding? Why should their places of concealment be prepared with fire? And why should the stratas of sand be introduced in their covering mound on Paint Creek, and human remains be burned upon them in Illinois? And why should mounds of such magnitude be necessary to insure temporary safety?

Again, supposing the disks to have been stored in their earthen warehouses to be withdrawn when needed, and were so utilized, we should find gaps and vacancies in the deposits where some had at different times been taken out. But such has not been the case. Mr. Squier and Mr. Moorehead assure us that the sand stratas placed — as winding sheets — over the flints in the Paint Creek mound had never been broken. The great deposits of these peculiar disks that

it has been my fortune to examine *in situ* on the Illinois River bore unmistakable evidence of high antiquity, and the primal integrity of their burial had certainly never before been infringed. I am reasonably sure that none of the flints placed there had ever been abstracted to be "worked over."

If the deposits had been only the hidden stocks in trade of enterprising implement makers carried to a distant market destitute of flint — as is Central Illinois — for trade or barter, we should reasonably expect the venturesome traders to have been well patronized by the dusky hunters and warriors there, with whom flint was an article of prime necessity. And we should also expect the Illinois consumers of this imported hornstone to have left evidences of their traffic in local workshops where the "roughed-out ingots" withdrawn from the great mounds had been worked down to finished weapons; and, as a consequence, we ought to meet there, in the surface finds, many of the finished implements of hornstone. But, as far as I can learn, no such hornstone workshops, abounding in dark-colored flint chips and broken disks, have ever been found in Central Illinois; not a hornstone disk like those in the deposits has ever been found there on the surface or isolated; and, of all the surface finds of flint relics in the Illinois River valley, less than *three* per cent are of dark colored flint and scarcely *one* per cent are of hornstone. From these facts, taken in connection with the undisturbed condition of the deposits, it must be inferred that as commercial speculations the importations of disks to the Scioto and Illinois, from a financial standpoint, were signal failures, as none of the stock was ever disposed of.

With this commercial idea in view, we would look with confidence for large deposits of hornstone disks in the Cumberland Valley and throughout Middle Tennessee, as fully *eighty-five* per cent of all flint implements found in the stone graves and on the surface in that region are of dark colored stone, and very many of them hornstone; those made of *white* flint being exceptional. But no deposits of "roughed-out ingots," as those in the Paint Creek mound, have ever been found in that State.

Those who regard the great deposits of flints under consideration as only hidden stores of blocked-out raw material, to be further manipulated when necessary or convenient, must admit that *as such* they are quite unique and without parallel among American prehistoric relics. And they must marvel that a people so provident and thrifty as to have made these thousands of "ingots" at the expense of so much time and toil, and transported them amazing distances, and *cached* them for future use, should not also have hidden away great stores of other kinds of raw materials, as copper nuggets "in the rough" to be worked over when needed; or quantities of marine shells to be withdrawn for the best market; or blocks of mica to be taken out and exfoliated at leisure; or "roughed-out" blocks of obsidian to be finished up on rainy days. But no magazines of unfinished goods are ever found; and the inference is unavoidable that the pre-Columbian tariff on raw materials was prohibitory, hornstone disks alone being on the free list.

My own limited observations in the field, and all the mound exploring literature to which I have had access, establish the fact *without exception*, in my opinion, that no primal deposit of any kind placed at the base of a mound has ever been subsequently disturbed by the people who made it. Hence, I think, we

may regard it *an axiom in the science of American archæology, that all original mound interments were by the mound makers considered sacred, and intended to be complete and final, never again to be reopened.*

Then, in light of the crude religious incentives — the spirit worship and demon propitiation, the vague hopes of blissful immortality and the frenzied dread of supernal forces, factors in the nebulous ethics of all tribes of the aboriginal American race — that moved the mound-building Indians to sacrifice in fire, without reserve, their choicest and rarest implements and ornaments brought from remote regions and wrought with wonderful skill — their splendid obsidian blades from Mexico; their shells from the far distant ocean; their finely cut plates of mica from the Appalachian or Rocky Mountains; their celts, and numerous ornaments of hammered copper from Isle Royale; their pipes of exquisite design, sculptured from the most refractory rocks; their polished effigies and rings of graphitic slate; and, no doubt, their bows and arrows, rich furs, and wearing apparel also, wholly consumed; — objects which they must have valued next to life itself; — we cannot, I think, escape the conviction that the motive prompting the making, transporting, and burying of the hornstone disks must have been superstition, and not economy.

My conclusions, therefore, are as follows: —

The buried disks were finished canoe-making tools, but not for use in this world.

That this variety of flint, "hornstone," perhaps on account of its color and gloss, its ready fracture and uniform cleavage, its nuclei of quartz crystals or chalcedony, or other intrinsic properties, possessed in the estimation of the mound-making Indians peculiar talismanic virtues.

That this stone, therefore, fashioned into canoe scrapers, they hoped would be most acceptable as votive offerings to the mythical powers controlling the navigation of the streams or their supplies of fish.

That their burial in the ground or in the mounds, whether in commemoration of extraordinary events, or — much more probably — as propitiating sacrifices, was sacred and final.

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INDIAN MIGRATIONS. By C. STANILAND WAKE, Chicago, Ill.

[ABSTRACT.]

THE distribution of the linguistic families of the North American area (excluding Mexico), as exhibited among the modern Indians, is shown with great exactness by the map prepared by Major Powell, and published in the Seventh Report of the Bureau of Ethnology. The data on which the map is based evidence the existence of special relations such as justify the grouping together of particular tribes, so as to form what may be called peoples or nations, although they may be insufficient for the purposes of a wider generalization. In the extreme north we find the Eskimos, who have to the south of them the Athapascan tribes on the west and the Algonkins on the east; the last named peoples spreading westward as far as the Rocky Mountains, thus adjoining the Athapascans on the North. Formerly the Algonkin tribes occupied most of the central region east of the Mississippi River, being separated from the Gulf

of Mexico only by the Muskhogean tribes, between whom and the Algonkins on the southeast were the Cherokees and other branches of the Iroquoian and Siouian families. The other members of the Iroquoian family occupied a large area around Lakes Erie and Ontario, and along the course of the River St. Lawrence, including nearly the whole of the present State of New York and a large part of Pennsylvania. West of the Algonkins are the Siouian tribes, who occupied the country on both sides of the Mississippi, their area being broken by tribes of the Caddoan family, which separate them from the Gulf of Mexico. West of the Siouian tribes the country is occupied chiefly by the members of the Shoshonian family, to the south of them being Athapascan tribes that have wandered from their original area, and to the north, separating them from the chief Athapascan area, the Salishan and Sahaptian tribes. Along the Pacific coast are numerous tribes more or less closely related among themselves, while to the north in California and in parts of Arizona and New Mexico are the Yuman, Piman, and other allied tribes. Finally, in Wyoming and Nebraska, south of the westernmost Algonkins, are situate the Kiowans, south of whom again are certain Algonkin tribes, the Cheyennes and Arapahos, who became separated from their kin under hostile pressure.

The position of the Arapahos and Cheyennes may be taken as illustrative of what has happened from time to time throughout the whole of the North American area. Although Major Powell's map may be regarded as giving the relative positions of the native peoples when the European explorers first came in contact with them, yet it may be safely said that the map would assume a totally different aspect if they were referred to their original habitats, in accordance with native tradition. It is of course a question how far these traditions are to be relied on, but migration stories are so general that they may be taken as establishing the general fact that few if any of the tribes now occupy the place in which they were originally located. Moreover, a comparison of traditions of different peoples may supply confirmatory evidence of the general truth of particular stories, and the object of the present paper is to see what light they may be supposed to throw on the migrations which have occurred throughout the North American area among the Indians belonging to the long-headed stock. The subject is a very wide one, and its treatment will necessarily be very inadequate within the present limits, but we may nevertheless arrive at certain positive conclusions.

Beginning with the Sioux-Dakotahs, a people who may be regarded as occupying a central position on the continent, we find that, according to certain traditions referred to by Mr. J. Owen Dorsey in his work on "Omaha Sociology," printed in the Third Annual Report of the Bureau of Ethnology (1881–82), the ancestors of the Omahas, Ponkas, Osages, and several cognate tribes, were at one time located near the River Ohio, down which they travelled until they reached its junction with the Mississippi, where they separated, the Omahas, Ponkas, Osages, and Kansas going up the Mississippi and subsequently spreading over the country between the Mississippi and Osage Rivers. At the mouth of the Osage the Omahas and Ponkas (now accompanied by the Iowas) crossed the Missouri, and passed gradually through Missouri, Iowa, and Minnesota to the neighborhood of the Red Pipestone quarry. Thence they went towards the Big Sioux River, where they remained a long

time, erecting a fort and building earth lodges and cultivating the ground. Subsequently the Dakotahs made war on the three tribes, killing many of them, and finally they went west and southwest, and after various wanderings reached Nebraska.

As bearing on this migration of the Omahas and Ponkas, reference may be made to a Sioux tradition, given by Mr. Gerard Fowke in his "Popular Errors in regard to Mound Builders and Indians," published in the Ohio Archæological and Historical Quarterly for December, 1888, which states that the Sioux were driven westward by the Chippeways, and that they found in Wisconsin, east of Lake Eau Claire, and Northern Minnesota, a people whom they called Ground House Indians, because they lived in houses of wood covered with mounds of earth. These Indians are said to have been large and strong, but cowardly, and they were driven away or exterminated by the Sioux. The nature of the habitations of the Ground House Indians and their location would seem to identify them with the Omahas and their allies, and the westward movement of the Sioux tribe is consistent with the general migration of the Sioux-Dakotah people, such as is suggested by the traditions obtained by Mr. Dorsey, under pressure from an encroaching people, who are identifiable through the Chippeways with the Algonkins.

Dr. Brinton tells us that "both Chippeways and Crees have been steadily pressing westward since the country was first explored, driving before them the Blackfeet and Dakotahs."[1] There may be some doubt as to the original dwelling place of the Blackfeet, or Sitsika, who, although usually classed as belonging to the Algonkin family, are claimed by Mr. Jean L'Heureux, the Canadian Government Interpreter of the Blackfeet Indians, as being of remote Nahuan origin, and as being connected by their religious ideas with the region where they dwell;[2] but not as to the Sioux-Dakotah, whose early home was probably in the valleys of the Ohio. This would account for the fact, pointed out by Mr. James Mooney, that there is a resemblance between the myths of the Cherokees and those of the Omahas and Poncas.[3] For the Cherokees, who are spoken of by Mr. Mooney as having been lords "of the whole country from the Savannah to the Ohio," are referred to in traditions relating the story of Algonkin migration as occupying a region, if not in Ohio itself, yet immediately adjacent. At least such is the case if, as there appears to be good reason for doing, the Talega or Tallegewi of the Walam Olam are identified with the Cherokees. The remembrance of this people having once lived in the Ohio Valley was kept alive by a Cherokee tradition, repeated annually in public by their official orators at the festival of the green corn dance. The Cherokees claimed to have been the constructors of the Grave Creek and other earthworks there. Dr. Brinton thinks this was about the fourteenth century, and he states in his edition of the Walam Olam that "they were driven southwards by their warlike neighbors, locating their council fire first near Monticello, Virginia, and their main body reaching East Tennessee about the close of the fifteenth century. As late as 1730 some of

[1] "Indian Migrations as evidenced by Language." The American Antiquary, Vol. V., 1883.

[2] Journal Anthropological Institute, 1885, pp. 161, 301.

[3] Journal of American Folk-Lore, Vol. I.

them continued to live east of the Alleghanies, while, on the other hand, it is evident from the proper names preserved by the chroniclers of De Soto's expedition (1542) that at that period others held the mountains of North Georgia." He adds: "To the Delawares they remained Kit-tawa-wi, inhabitants of the great wilderness of Southern Ohio and Kentucky." The Cherokees must formerly, however, have occupied the country as high north as Lake Erie, for Heckewelder refers to fortifications built by them about twenty miles east of Detroit, and others on the Huron River, six or eight miles from Lake Erie. Outside of the fortifications his native guide pointed out a number of large mounds where were said to be buried hundreds of the Tallegewi who were slain in the fights spoken of by the Algonkin tradition preserved in the Walam Olam.

The Cherokees are thus brought into the region which traditions preserved by the Sioux-Dakotah tribes speak of as their early home, and which afterwards became part of the domain of tribes belonging to the Iroquoian family. It is a remarkable fact, that, according to the researches of Mr. Horatio Hale, the Cherokee language belongs by its grammatical structure to the Huron-Iroquoian group, — a relationship which is confirmed by Mr. A. S. Gatschet. This relationship may perhaps account for the conduct of the Mengwe or Iroquois of Heckewelder's tradition, who refused at first to join the Algonkin or Lenape in their attack on the Tallegewi, and hung back in the actual fighting, which they might well do if they and the Tallegewi were of kin. It should be noted, however, that according to Gallatin,[1] the Hurons, who were allied to the tribes known to history as the Iroquois, claimed the country between Lake Erie and the River Ohio, and their right was never disputed except by the Eastern Iroquois; so that the Hurons may have been the real allies of the Lenape against the Tallegewi, or at least they may have at one time occupied that region. As to the Eastern Iroquois, their tradition of origin, as told by Mr. Lewis H. Morgan, relates that prior to the occupation of New York State "they resided in the vicinity of Montreal, upon the northern bank of the St. Lawrence, where they lived in subjection to the Adirondacks, a branch of the Algonkin race, then in possession of the whole country north of that river. At that time the Iroquois were but one nation, and few in number. From the Adirondacks they learned the art of husbandry, and while associated with them became inured to the hardships of the chase. After they had multiplied in numbers and improved by experience, they made an attempt to secure the independent possession of the country they occupied, but, having been in the struggle overpowered and vanquished by the Adirondacks, they were compelled to retire from the country to escape extermination."

This was the usual course of Indian history, and we find among the peoples mentioned in the Walam Olam as having been driven away by the Algonkin invaders the Snakes, who are said to have been "weak, and hid themselves in the swamps." Dr. Brinton, in referring to the suggestion that the Tallike or Cherokee were the mound-builders of Northern Ohio, remarks: "The inference rather is that the Snake people, *Akowini* or *Akonapi*, dwelt in the valleys north of the Ohio River, in the corner of Western Ohio and Indiana, where the most

[1] Arch. Amer., II. 69.

important earthworks are found,—and singularly enough, none more remarkable than the immense effigy of the serpent in Adams County, which winds its gigantic coils over seven hundred feet in length on the summit of a bold bluff overlooking Brush Creek." Possibly the Creeks might be identified as the Snake people, seeing that the Lenape are said by the Walam Olam to have fought in the land of the Talega and Koweta, the former of these being the Cherokees and the latter the Creeks. But this war was as late as the historical period, whereas the encounter with the Akowini or Akonapi took place before the Lenape reached the Tallegewi. The Snake people are more probably, therefore, to be identified with a Sioux-Dakotah tribe, and as a fact that was the name given by the Algonkins to the Sioux, this term being apparently a corruption of an Algonkin word meaning "the snake-like one." The Snake people are in one passage of the Walam Olam associated with the *Assigunaik* or "Stone men," who may be identified perhaps with the Sioux tribe Assineboin, or Stone Indians, so called from their custom of boiling food by means of hot stones.

Let us now see what light is thrown on the place of origin of the Algonkins by the tradition preserved in the Walam Olam. We read there, according to Dr. Brinton's reconstruction of the ancient history of the Lenape, that "at some remote period their ancestors dwelt far to the northeast, on tide water, probably at Labrador. They journeyed south and west, till they reached a broad water full of islands abounding in fish, perhaps the St. Lawrence about the Thousand Isles. They crossed and dwelt for some generations in the pine and hemlock regions of New York, fighting more or less with the Snake people and the Talega, agricultural nations living in stationary villages to the southeast of them, in the area of Ohio and Indiana. They drove out the former, but the latter remained on the Upper Ohio and its branches. The Lenape now settled on the streams in Indiana wished to remove to the east to join the Mohegans and others of their kin, who had moved there directly from New York. They therefore united with the Hurons (Talamatans) to drive out the Talaga (Tsalaki, Cherokees) from the Upper Ohio. This they only succeeded in accomplishing finally in the historic period. But they did cleave the road and reached the Delaware Valley, though never forgetting nor giving up their claims to the western territories." Dr. Brinton thinks the tradition of the Walam Olam older than that given by Heckewelder, and he sees a confirmation of it in the tradition of the Western Algonkins that their common ancestors lived north of the St. Lawrence near the site of Montreal. It is very improbable that the original home of the Western Algonkin tribes should have been in that region, while the Delawares originated west of the Mississippi, as supposed by Heckewelder, owing to his identifying the Namaesi-Sipu, or River of Fish, with the Mississippi instead of with the St. Lawrence, as proposed by Dr. Brinton for the Nemassipi. The two traditions referred to are strictly parallel after this point, and if Heckewelder's is a later one it may have been affected by the vicinity of the tribes who possessed it to the Mississippi.

The Western Algonkins retained a remembrance of their origin north of the St. Lawrence, and the fact may be considered as established that the Algonkin people, as we have seen to have been the case also not only with the Iroquois and the Cherokees but with the Sioux-Dakotahs, dwelt at an early date near

the St. Lawrence, that is, on the eastern side of the North American continent. This fact is the more important, as all those peoples belong to the long-headed or dolichocephalic type, as distinguished from the Indians of the western coast, who belong as a rule to the brachycephalic type, to which the earlier peoples of the Mississippi Valley, referred to as the mound-builders, appear also to have belonged. It is thus not improbable that all the peoples brought together by their traditions in the neighborhood of the Eastern Lakes had a common origin. The place of this origin may well have been north of the St. Lawrence and the Great Lakes, and there is nothing unlikely in the story related by the French traveller Du Pratz, and which is accepted as genuine by De Quatrefages, of the journey beyond the Rocky Mountains to the Northwest Coast undertaken by a Southern Indian in search of the ancestral home of the "Red Men" of the North. This story would bring the ancestors of the Northern Indians in contact with the Athapascans, who are said to belong to the dolichocephalic type, and who have various points of of affinity with the Algonkins and with the Eskimos, who adjoin both the Algonkins and the Athapascans to the north, and also belong to that cranial type.

Notwithstanding their evident relationship to the other division of the American aborigines, those who occupy what may be described generally as the continental area west of the Rocky Mountains, it is almost certain that the Eastern tribes owe their dolichocephalic character to the influence of a race or races belonging to a different stock. The dolichocephalic peoples who now appear to be the nearest to the American dolichocephalous area are those of Europe and Africa, but it is possible that their closest allies are to be sought within the Pacific area. This would require either that the primitive dolichocephalic stock of North America spread originally over the continental area from some part of the Northwest Coast, or that the foreign element to which they owe their special characters was introduced there. That the American Indian, whether dolichocephalic or brachycephalic, has a closer physical resemblance to Asiatic peoples, with whom must be classed those of the Pacific area, than with either Europeans or Africans, can hardly be denied. The brachycephalous American presents many points of analogy to the Mongols, and the Islanders of the Pacific are said by Dr. Topinard to resemble the typical Eastern Indian in certain features. Possibly such characteristics may have been derived from an earlier people, who are known to have occupied some of the islands of the Pacific before the arrival of the Polynesian Islanders.

It is probable, indeed, that all the present peoples of the Pacific area, including the adjoining regions of the Asiatic continent, have been affected more or less by a primitive dolichocephalic element, which finds its nearest representative on the American continent in the Eskimo, whose type of skull was shown by Welcher to approximate to the scaphocephalic character. This has since been found among the Caroline Islanders and the Fijians, and even among the aborigines of Australia. It is described by Topinard as a deformity of the cranium, "characterized by its contraction transversely, its antero-position elongation, and its increase in height. The skull turned upside down has the form of a boat, from which its name is derived; the forehead is straight, bulging, and narrow; the occiput is globular and conical, and projects backwards from the lambdoidal suture. A horizontal crest reaches from one side

to the other on the anterior half, the sides shelving like the roof of a house, which the obliteration of the parietal protuberances renders still more prominent."

That the Algonkin skull possesses particular points of resemblance to that of the Eskimo was long since pointed out by Dr. Prichard, and hence the long-headed American type may be supposed to have obtained its special features through the Eskimo, whose influence has affected even the aborigines of South America, as shown by the fact, pointed out by Professors Canestrini and Moschen,[1] that the Botomdo of Brazil are connected by their orbital index with the Eskimo, who may be themselves the product of the superposition of the Pacific dolichocephalous stock on the stock from which the American brachycephali were derived.

As to the origin of the American short-headed peoples, I will say only, in conclusion, that it evidently has near Mongolian affinities, but that both the brachycephalous American and the Mongolian are more probably representatives of the primitive man of Central Asia, than that either of them has been derived from the other.

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PSYCHOLOGY AT THE WORLD'S FAIR. By Prof. JOSEPH JASTROW, Madison, Wis.

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SOME DRAWINGS BY KOOTENAY INDIANS. By Dr. A. F. CHAMBERLAIN, Clark University, Worcester, Mass.

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REMARKS UPON THE SHEET COPPER DESIGNS OF THE HOPEWELL GROUP, OHIO. By WARREN K. MOOREHEAD, Zenia, Ohio.

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A CEREMONY OF THE QUICHUA INDIANS, PERU. By GEO. A. DORSEY, Assistant, Peabody Museum, Cambridge, Mass.

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THE ANCIENT NECROPOLIS OF ANCON, PERU. By GEO. A. DORSEY, Assistant, Peabody Museum, Cambridge, Mass.

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NOTES FOR AN ARCHÆOLOGICAL STUDY OF LA PLATA ISLAND, ECUADOR. By GEO. A. DORSEY, Assistant, Peabody Museum, Cambridge, Mass.

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INDIAN NAMES FOR THE FOUR WINDS AND FOUR QUARTERS. By Dr. J. OWEN DORSEY, Tacoma Park, D. C.

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THE EVIDENCE FOR GLACIAL MAN IN NORTH AMERICA. By Rev. G. F. WRIGHT, Oberlin, Ohio.

[1] Sopra due Crani di Botomdi, p. 16.

EVIDENCE OF HUMAN ANTIQUITY. By W. J. McGEE, Bureau of Ethnology, Washington, D. C.

SOME FACTS CONCERNING THE OBSIDIAN BLADES CALLED SWORDS, FROM NORTHERN CALIFORNIA. By HORATIO N. RUST, South Passadena, Cal.

SOME ACCOUNT OF A PURIFICATION CEREMONY AND A SACRED STONE IN USE AMONG THE MISSION INDIANS OF SOUTH CALIFORNIA. By HORATIO N. RUST, South Passadena, Cal.

THE INDIAN STONE ADZE OF NORTHERN CALIFORNIA. By HORATIO N. RUST, South Passadena, Cal.

THE RELATION BETWEEN MYTHOPEIA AND EUHEMERISM. By Rev. MERWIN MARIE SNELL, Chicago, Ill.

NOTES TAKEN AT THE NEOLITHIC FLINT QUARRY OF SPIENNES, BELGIUM, IN MARCH, 1893. By H. C. MERCER, Doylestown, Pa.

REVISION OF CALENDAR. By ADA M. KING, Rochester, N. Y.

AN ANCIENT SHAWNEE TOWN AND ITS EXPLORATION. By W. E. MYER.

GI-YE-WA-NO-US-QUA-GO-WA. (THE SACRIFICE OF THE WHITE DOG.) By C. A. HIRSCHFELDER, Toronto, Canada.

# SECTION I.

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# ADDRESS

BY

# WILLIAM H. BREWER,

VICE PRESIDENT, SECTION I.

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## *THE MUTUAL RELATIONS OF SCIENCE AND STOCK BREEDING.*

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THE production of crops and the production of animals are the two great branches of Agriculture. The modern application of science to the economic production of plants has been so much more conspicuously before the public than its application in the economic production of animals, that I have thought it might be well, on this occasion, to consider the latter.

The application of scientific methods to the solving of economic problems constitutes perhaps the most distinctive feature of modern intellectual progress, and in no industry has this been more significant than the revolution which science has produced in the art of Agriculture, for within about a century greater changes have taken place than in all the centuries from the dawn of history.

In the production of cultivated plants the great change has been in lessening the labor of tillage, in harvesting the crops, in handling the product, increasing in the number and kinds of species and varieties cultivated, in fertilizing the soil, and in transportation. These have come about mostly through the science of chemistry and the growth of mechanical invention. The economic effect has been to diminish the number of men required to produce a given amount of crop; and to lessen enormously the amount of capital employed in agricultural production as related to the amount invested in other industries. It is only fifty years ago that Johnstone, the eminent writer on Agricultural Chemistry, asserted that nine tenths of the working capital of the civilized world was in-

vested in Agriculture or in handling its products. Now, perhaps, not one third is thus employed, and the ratio is continually diminishing.

In the breeding and rearing of domestic animals, science has worked somewhat differently. Economic results are following more tardily and from a somewhat different direction. Biological rather than the strictly physical sciences are the leading factors, and Biology is of later growth and application than Chemistry and Physics.

There could be no science of Agriculture worthy of the name until we had a science of Chemistry, and very soon after this was placed on a sound foundation by the promulgation of the atomic theory, and chemical analysis was established on a scientific basis, works on Agricultural Chemistry as an applied science appeared, and then we began to have a science of Agriculture.

Not so with breeding. It remained strictly an art until very much later. The production of useful animals is one of the oldest industries, as it is one of the most widely spread, and, as an art, it attained a high degree of efficiency at a very early date, but has only risen to the dignity of a science since the publication of Darwin's "Origin of Species." There is no proof that the art advanced much from the days of Cain, the earliest breeder in history, down to the last quarter of the last century. About that time, and just as several of the physical sciences were being established on a philosophical foundation, experiments in breeding, carried on more especially in England, did much to advance the art, and laid the foundations upon which a science is now being constructed. There was, however, no obvious connection between the scientific awakening and the breeding experiments; they merely belonged to the same period.

During the ages preceding the Christian era mankind had experimented on the taming and training of animals for various uses, and had already domesticated the few species now used for this purpose. During this long period, artificial breeding had changed the nature of the wild originals into the several characters essential to domestic animals. Man had made them perfectly fertile in captivity, had established the instinct of tameness, and had bred into them that variability and plasticity which adapted them to the artificial environment which he imposed. This was so slow a process that none of the more important species have been domesticated since.

The process is such a slow one that time and capital are now too valuable. The turkey had already been domesticated by the Mexicans before it was introduced into Europe, so we may say that the canary bird is about the only modern domestication, unless it be the ostrich, which we are now beginning to breed.

The breeding of domestic animals may be arranged into three illy defined classes: — those bred merely for use, of which swine and asses may be taken as examples; those bred for social and ceremonial purposes as well as for economical use, as horses, dogs, and game fowls; and those bred for pure fancy, as fancy pigeons, canaries, certain varieties of pet dogs, and white mice. The general laws underlying their production are the same, but the details of the art are very different.

The production of new varieties as well as the production of the very best examples of each breed or sort in practice still remains strictly an art. Modern science has aided us comparatively little in this. What it has done has been to place the general economic production on a much surer basis, and make results more certain by greatly lessening the element of chance. Let us now note the way this progress came about. Selection was the foundation of all the work, but until the last century purity of blood in the sense in which we now use the term played a secondary part. Except with certain strains of Arabian horses, and possibly of game fowls, it was considered of minor importance. The inheritance of acquired characters was assumed as a fact by all breeders; although it was not believed to be the most important factor, it was one to be considered.

Although there was no science of breeding, there was a large literature produced before the beginning of this century relating more or less directly to the art. For social reasons literature relating to horses and dogs is much more voluminous than that relating to the other domestic animals, but there is considerable devoted to both useful and fancy breeding.

The rules laid down are almost as numerous as the authors, and wild speculations are freely mingled with records of observation. Good parentage is always recommended, but "blood," in the modern sense of the word, was almost unknown, or at least was practically ignored. The general principle that ancestry is more important than the special excellence of the immediate parents does not appear to have been known to most writers, and the selec-

tion of animals for pairing depended entirely upon the sagacity and experience of the breeder.

It was then as well known as now that cross-bred animals were often superior to either of the parents. It was, therefore, but natural that crossing should be the favorite method in the attempts to improve useful live stock.

Improvement within the breed, keeping the blood pure, was not pursued in Europe until the last part of the last century. The great change in practice began in England first in breeding race-horses. It was some time later before it extended to the breeding of cattle, sheep, and other species.

Horse-racing was a favorite sport with the higher social classes in England from early times, and long before the beginning of the last century horses for racing were imported from Oriental countries and from various countries of Europe, more especially from Spain. From the mixture of these varied stocks the present English Thoroughbred originated. The Racing Calendar was begun in 1727, and pedigrees of the winners began to be published soon after. After a time the sporting periodicals also published a list of the "winning sires"; that is, a list of the stallions whose progeny had won the races of that year, along with the names of the winners and the amount of the winnings. These gave data for the selection of sires in breeding for speed.

Cheny proposed publishing a list of pedigrees of famous horses in 1741, but this was not done as a separate publication until Weatherby collected the pedigrees as far back as there were any data, and published the first volume of the Stud-Book in 1791.

This was the beginning of the means for founding a science of breeding. There could be no science, in the modern sense of the word, until there was a systematic record of the facts essential for study published and available to students, and from which generalizations could be deduced. No law of heredity could be established until we had the means of studying all the ancestors of an animal for at least three or four generations back. The Stud-Book furnished the means for such a study of the animals of one breed. From the data thus published it came to be noticed that the "winning sires" were, as a rule, those of the purest blood, but this rule in breeding was not extensively practised in the producing of farm animals until long after. There was no publication of any record of pedigrees of animals other than horses for more than

thirty years later. The "Shorthorn Herd-Book" was published in 1822.

Remember, in this connection, that the leading breeds of domestic animals had been established as to general type long before this, and partial pedigrees were kept by some of the more successful breeders. But these were manuscript records, kept merely as aids in the private practice of a secret art. Until long after the publication of the Stud-Book and the Shorthorn Herd-Book there was no science of breeding recognized.

The wide and capricious variation induced by crossing must have been noticed from very early times, and, to lessen its uncertainties, breeders in the last third of the last century began to practise extensively the opposite extreme. Then the term as well as the practice of breeding in-and-in came into practical use in Great Britain. It was the forerunner of breeding by pedigree applied to other animals than race-horses. Its great promoter was Robert Bakewell, who began his experiments about 1775. Beginning with a very few choice animals, he grew his flocks and herds from them, breeding between the nearest of kin. The ends gained by this were greater uniformity of excellence and increased prepotency. He selected with rare judgment and wrought a marvellous improvement in his animals. The changes extended to form, quality of flesh, early maturity, and general useful qualities. He wrote nothing. With him breeding was a secret art, but, fortunately, he taught this art to certain pupils, of which the brothers Colling became famous as breeders of shorthorns. Their success was so great that at a sale in 1812 one of their bulls sold for a thousand guineas, the first time that so large a price had ever been paid for any animal of the ox kind. Through breeding in-and-in, "fixity of type" became common as a breeder's term for breeding true, and then, naturally, pedigree became the basis for breeding.

Still, the principles of breeding were not yet understood. Even Colling introduced a cross into his herd, and breeders to this day are discussing the influence of that "Galloway cross" on the shorthorn breed.

Sir John Seabright, of whom Darwin says, "the monument to whose genius exists in the Seabright Bantam," was one of the clearest of the early writers, and by some is considered the father of the science. In a letter published in 1807, he says: "Were I to define what is called the art of breeding, I should say that it

consisted in the selecting of males and females to breed together in reference to each other's merits and defects." He then qualifies this at length, stating that it is not necessarily by pairing the best male with the best female that the best results are produced, as they may augment some defect which another cross would decrease, but he does not appear to have understood the relations of this to the more remote ancestry. This only came to be understood later by a study of published pedigrees, and after Darwin pointed out certain causes which induced variation, and coined that word "reversion" now in such common use by breeders.

Most of the leading breeds of our farm animals existed, after a fashion, in the last century. The early history of nearly all of them is obscure, although much research has been expended in modern times to unravel it. But unless confined to some small island, as were the Alderney, Jersey, and Guernsey cattle, the breeds were not kept pure, because the methods of improvement were by crossing with other blood rather than by the careful selection of pure-blooded animals of the same breed. Uniformity could not be secured by such practice, therefore all the economical results were very uncertain. The value of breed was recognized, but the value of pure blood was not, at least by breeders at large.

After pedigree records were systematically printed and studied, then improvement by selection within the breed became the rule. Herd-books were multiplied, and finally especial associations sprang into existence for the promotion of the several breeds. The quarter of a century during which Darwin was accumulating the data for his "Origin of Species" was an important one in the history of the theory of breeding, and the publication of pedigree records of one kind and another was then begun. But scientific naturalists, absorbed in the description of natural species, ignored man's artificial productions. A breed may be, and often is, as artificial as a picture or a statue. The breeder, like the artist, must have his ideal towards which he is working. The greater his genius, the nearer his creations come to reaching his ideal. They work by different methods and with very unlike materials, but both are artists. A noted breeder of sheep once said to me, while talking about his methods and success: "All real sheep have defects, none are perfect. How to breed out the defects and breed in the merits wished for, is the great problem. I can shut my eyes and see standing before me the sheep I have so long been trying to breed.

I can see it just as plainly as I can a real sheep, but," he added, sadly, "I have never been able to produce it." He did, however, produce wonderful creatures that were sold for more wonderful prices, animals that had a reputation among sheep growers all over the world. And his creations were as actually artistic productions as were those of Phidias.

The younger generation of naturalists, in these modern days, can hardly appreciate how completely the work of breeders was ignored by scientific naturalists during the half-century preceding the Darwinian era. The earlier naturalists, like Buffon and Cuvier, had treated and written of domestic animals as a part of nature's productions, but their successors came to look upon these "monstrosities" with great contempt. They conceded their economic importance, but put them outside of scientific investigation.

Nevertheless the times were ripe for the publication of a new theory on the origin and nature of species. The difficulties of the old system had accumulated until they were wellnigh crushing the life out of natural history. The distribution of species was a mystery without philosophical explanation, and the relation of species to varieties a source of perpetual discord and discussion. When Darwin brought us out of the difficulty it was largely by a study of the experiments made by breeders. This was analogous to introducing to the scientific naturalists of the world a new and vast biological laboratory for scientific experimentation.

It is in fancy breeding that the most wonderful results are obtained, and this Darwin pursued experimentally. Here the economic and useful factors are eliminated. With the creature thus evolved there is no struggle for existence. Man provides, protects, and sustains. There is often no reason for its being except man's whim. We now see societies and associations for the promoting of useful breeds of farm animals so numerous and so prominent that we forget that it was the "Fanciers" who first had such clubs or associations in the interest of their favorite breeds.

Here let me explain that all the earlier Stud-Books and Herd-Books were started by individuals as private business ventures, but later most of them came to be published by associations having much wider aims. Now, nearly every useful and important breed has an authoritative pedigree record of some kind, its "Stud-Book," "Herd-Book," "Flock-Book," or "Register," published under the auspices of an association formally endowed with authority. I have

no knowledge of how many such there are, but this much I may say, that in 1859, when the "Origin of Species" was published, there was not in the whole world a single society or association in existence for registering the pedigrees of the useful farm animals, or for defining the special and essential charaters of the several breeds. Nearly every useful breed has now an association for this end, and pedigree records run into the hundreds, if not thousands of volumes. The "Live Stock Journal," of London, stated in 1886 that in 1872, only fourteen years before, there was not one such society in the United Kingdom, while at the writing (1886) there were twenty-five. Nearly every breed in every country now has an authoritative association which, by the recording of pedigrees and the fixing of standards, dictates what the character shall be of those artistic creations called breeds. They are strictly analogous to those scientific bodies which make rules to govern nomenclature, establish units of measure, or dictate methods by which analyses, experiments, or observations shall be made.

But the breeders of fancy stock had their societies and set their standards very much earlier; how early I do not know. Fancy breeds of canaries had a "standard" and a "boom" long before any useful breed of farm animals had. I have seen references to the list of standard qualities of canary birds published by the "Hand-in-Hand Canary Society" as early as 1779, and twenty years later Hervieux, in his work on canaries, gives the price of birds, and which of the favorite breeds brought fancy prices, ranging up to one hundred and twenty-five dollars each. It is interesting to state in this connection, as illustrating the caprices of fashion in fancy breeding, that the breeds most desirable then have now so completely passed out of existence that it is uncertain what they were, or even what they nearest resembled. There were also many dog clubs and pigeon associations long before the days of Darwin. Many of you remember how he joined the various Pigeon Societies of Great Britain, and put up his cotes; and the interest with which he mingled with his fellow fanciers is indicated by many allusions in his life and letters.

The Saturday Review recently published a notice of Ure's work, "Our Fancy Pigeons." This notice, which was not intended to be scientific, expresses so well the sentiments that were formerly held by scientific men, as well as literators, that I will quote it. (It is to be understood that "The Fancy" has come to be a popular

name in England for "Fanciers," more particularly for the breeders of fancy pigeons.)

"There could not be a better name than 'The Fancy' for people who are absorbed in the contemplation of pigeons, have their minds crammed full of pigeons, and evolve from their inner consciousness most wonderful forms, which they actually succeed in producing by close attention to breeding. Some pigeons are so highly bred that they cannot fly; some cannot see; others that cannot eat; others that cannot walk; and others,— tumblers,— tumble themselves to death, so much has breeding done for them."

Referring to the frontispiece of the book it goes on to say that "to a poor, ignorant, unfanciful person this plate looks like an exaggerated picture of a monster of ugliness, but, so far from that being the real truth of the matter, it is a faithful likeness of one of the most beautiful and perfect Pouters that ever was bred."

No wonder that breeding had no scientific status when we consider that this was the attitude that naturalists as well as the ordinary public assumed towards pigeon fancying previous to the publication of the "Origin of Species." I need not remind my hearers of the great change that has occurred since, nor of the rich results to science that have come from the scientific study of these biological phenomena.

There is not a naturalist in the whole world to-day who would not be ashamed to confess such ignorance of the subject as most naturalists were then actually proud of. Artificial selection and what it may produce is now an essential part of the elementary study of every naturalist, and indeed is now studied to some degree by every educated person. I question if the doctrine of natural selection would have readily gained acceptance had it not been that the results of artificial selection were conspicuously before every one's eyes. Artificial selection and natural selection are in fact more unlike than is generally conceded, but they are sufficiently alike for very important scientific deductions, and the results are now freely used to explain theories in science.

Time forbids my following this line farther, but I do not think that I state the case too strongly when I say that a study of the art of breeding has resulted in a profounder change in scientific dogma than has ever been produced by the study of any other economic art.

Let us now glance again at the economic aspect of the subject. The first great effect of this scientific study has been to render

breeding for economic purposes more certain. Pure science is exact in the proportion in which it enables us to predict events; its economic applications are valuable in the proportion in which it enables us to control results. The breeder of to-day controls results with a success his ancestors never dreamed of. With mongrel stock without pedigree, there may be produced some animals of great individual excellence, the accidental result of uncontrolled and uncontrollable variation. With pedigree we may reasonably predict the production of good animals, not as the result of accident, but of design.

The economic production of animals is therefore now placed on a very much surer foundation. The chances for failure are enormously lessened, and the methods of improvement are founded on a philosophical basis. Characters are changed at will when economic considerations advise it. These are the days of "records," but until lately these records have affected breeding only in one direction, that of speed in horses. Now it is being applied to numerous other achievements and qualities. Milking tests are a factor in the breeding of dairy cattle; fattening qualities and early maturity, in the production of animals for flesh; size, prolificacy, color, and form,— in fact, any desirable characters, —are now bred for with promise of success.

When we consider the magnitude of our animal production and the intimate connection of domestic animals with our social life we get an inkling of the vastness of the subject and the economic importance of its being placed on a scientific foundation. In economic animal productions new breeds can be evolved in shorter time, old breeds quicker and more surely improved, relative qualities more accurately tested, and excellent animals more surely produced. All the economic results are already rendered more secure because of our better understanding of the laws of heredity and of the causes which promote variation.

There are numerous scientific and social problems which are yet to be settled, in which the data for the solution lie largely in this field. For example, the question now mooted by naturalists, the inheritance of acquired characters, if ever settled experimentally, will be settled by experiments on domestic animals. So, too, the production of new instincts, the ultimate effect of education and other social problems, can best be here studied experimentally. It is a universal laboratory for experiment and observation. No

other biological experiments are conducted on so vast a scale. Animals are bred in countless millions, for a great variety of purposes, by people of every race and grade of culture, and they are subjected to every condition of environment. The ultimate gain to pure science from the study of the great mass of data here available I will not attempt to predict.

# PAPERS READ.

SURFACE TENSION OF WATER AND EVAPORATION. By Dr. MANLY MILES, Lansing, Mich.

[ABSTRACT.]

THE surface tension of water is now attracting attention as an important factor in the metabolism and consequent fertility of drained soils, and it appears to be an appropriate subject for discussion in the section of economic science.

The purpose of the present paper is to make a preliminary report of the results obtained in some experiments now in progress to determine the effects of a stress in modifying the surface tension of water in contact with air, and the resulting influence on the rate of evaporation.

A convenient method of subjecting the surface film of water to a definite and controllable stress was suggested by a paper "On a Septum permeable to Water and impermeable to Air, with Applications to a Navigational Depth Gauge," read by Sir William Thompson at the British Association meeting in 1880. In this paper it was shown that "a small quantity of water in a capillary tube with both ends in the air acts as a perfectly air-tight plug against differences in pressure corresponding to the height at which water stands in the same capillary tube when it is held upright with one end in water and the other in air. And if the same capillary tube be held completely under water it is perfectly permeable to water, opposing no resistance except that due to viscosity, and permitting a current of water to flow through it with any difference of pressure at its two ends however small." It was also remarked that the same tube when not plugged by liquid was perfectly permeable to air.

The meshes of a piece of fine wire cloth or fine muslin are in effect capillary tubes, which when wet and exposed to the air, on one or both sides, are impermeable to air under a pressure corresponding to the height water will rise in a capillary tube of the same diameter as the meshes, while water is freely allowed to pass through them. The tension of the surface film of water in contact with air is the efficient factor in the phenomena here presented, and the method adopted for subjecting it to a stress is based on its properties of excluding air and permitting the free passage of water.

When a large tube, like the chimney of an argand burner, with a fine cloth tied over one end, has the other end dipped in water, the air readily passes through the meshes of the dry cloth as the tube is lowered into the water, and when it is entirely submerged the air has escaped and the tube is filled with water. When the tube is now raised, as the wet cloth comes in contact with

the air a surface film is formed, which excludes the air and in effect supports the column of water retained in the tube.

The surface film under these conditions is subjected to a stress from the weight of the suspended column of water, and this stress, which increases the surface tension, varies with the height of the column supported, which, for convenience, we shall call the pull. With tubes nearly five centimeters in diameter, covered at the upper end with a single thickness of fine muslin, the column of water has been suspended for thirty-six days under a pull of from twenty to twenty-four centimeters, or until from evaporation the pull was sufficient to overcome the resistance of the film to the admission of air.

Water poured on the muslin covering the top of the tube runs freely through without disturbing the suspended column of water, but a few drops of alcohol diminish the surface tension of the film, so that air is admitted and the column of water falls to the level of the water outside of the tube.

That the surface tension of the film closing the meshes of the cloth is increased with the pull was shown as follows. A disk of copper 1.5 centimeters in diameter, suspended from one arm of a delicate balance, was placed in contact with the film on the cloth at the top of the tube. When the surface of the water outside of the tube was on a level with the muslin or bolting cloth covering its upper end, a weight on the opposite arm of the balance of about 0.8 of a gram was required to separate the disk from the film, which represents the surface tension of the water. When the water outside the tube was lowered to subject the film to a stress, a larger weight was required to raise the disk from the film; and under the conditions of the apparatus as tested an increase in the weight of about 0.8 of a gram was required to raise the disk for each increase of 0.5 of a centimeter in the pull. Slight variations from these averages from day to day suggested the influence of atmospheric or other conditions which have not as yet been determined.

In my experiments on evaporation, which have been continued for several months, bolting cloth of about 140 meshes to the inch was tied over the upper end of argand lamp chimneys of uniform size, and the edges secured with shellac varnish. These tubes were then suspended in tall, slender beakers, so that the bolting cloth would be submerged when the beakers were full. The beakers were then filled with distilled water, the air in the tubes escaping as the water rises until the bolting cloth is completely covered.

The water in the beaker is then lowered by means of a rubber bulb, and a column of water remains suspended in the tube, as the air is excluded by the surface film which plugs the meshes of the bolting cloth. The pull is then readily varied by increasing or diminishing the water in the beaker. The pull in centimeters and the amount of water evaporated in cubic centimèters were shown by graduated scales on the sides of the beaker.

The water surface between the outside tubes and the sides of the beakers was then covered with oil, so that evaporation of the water could only take place from the liquid film on the surface of the bolting cloth at the top of the tubes.

Control experiments were made with two similar tubes closed at the bottom with cork, and kept full of water by adding measured quantities from day to day to replace the loss from evaporation. A naked water surface was exposed

in one of these tubes, while the other was loosely covered with bolting cloth supported and retained at the surface by a submerged float.

The evaporation from the tubes in which the surface film was subjected to a stress by the pull of the suspended column of water was, on the average, about fifty per cent greater than from either of the control tubes in which the surface film was not subjected to a stress. A single example will illustrate the influence of the stress in promoting evaporation. The evaporation for 15 days from the different surfaces was as follows: from the cloth covered surface, not under stress, 73 c. c.; from the naked water surface, 64 c. c.; from the surface subjected to a pull of 0.9 to 2.1 centimeters, 106 c. c.; and with a pull of from 8 8 to 11.5 centimeters it was 125 c. c.

Thus far the indications are that the evaporation from the surfaces subjected to a stress is largely in excess of that from a naked water surface, or from one covered with cloth; but there are considerable variations in the amount of this excess, and it remains to be seen to what extent these variations are caused by atmospheric conditions of pressure and moisture, in connection with differences in stress, which have not as yet been fully tabulated to make them strictly comparable.

The difficulties in the way of determining the relations of atmospheric conditions to the slight variations in surface tension and rate of evaporation that have been observed, arise in the main from differences in the accidental contamination of the evaporating surfaces that are liable to occur in various ways, and more extended observations are required to ascertain their significance.

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ENERGY AS A FACTOR IN NUTRITION. By Dr. MANLY MILES, Lansing, Mich.

[ABSTRACT.]

IN a paper on "Energy as a Factor in Rural Economy," read in Section I at the Washington meeting of the Association, I made a quantitative estimate of the energy expended in the exhalation of plants and in evaporation from the soil. As a supplement to that paper, quantitative estimates of the potential or stored energy of an acre of corn and of a fat ox are now presented as approximate indications of the amount of energy required in the constructive processes of nutrition in plants and animals.

Some of the leading facts are summarized in a tabular form for convenience of reference.

In general terms the food of plants may be said to consist of the simple chemical elements and their binary compounds; and the results of a chemical analysis of an acre of corn may be presented as in division A of the table, in which the elements of its composition are alone considered. These elements may be interpreted as representing the proximate composition of the crop, and the results of the analysis would then be given as in division B of the table. This does not, however, represent the whole truth in regard to its composition, as one of the most significant factors in its processes of nutrition, and nutritive value as food, is entirely neglected.

| | | Corn. 1 acre, grain, 3360 lbs.; stalks, 3840 lbs. | | Fat ox. Live weight, 1419 lbs. Stomach, contents, etc., 85 lbs. | |
|---|---|---|---|---|---|
| | | % | lbs. | % | lbs. |
| A | Water | 17.1 | 1232 | 45.5 | 646 |
| | Carbon | 39.7 | 2858 | 31.6 | 448 |
| | Hydrogen | 5.1 | 367 | 4.6 | 65 |
| | Oxygen | 33.6 | 2416 | 6.1 | 87 |
| | Nitrogen | 1.3 | 90 | 2.4 | 34 |
| | Ash | 3.3 | 237 | 3.9 | 55 |
| B | Water | 17.1 | 1232 | 45.5 | 646 |
| | Proteids | 7.8 | 562 | 14.5 | 206 |
| | Fat | 3.3 | 237 | 30.1 | 427 |
| | Carbohydrates | 68.5 | 4932 | | |
| | Ash | 3.3 | 237 | 3.9 | 55 |
| | | Energy. | | | |
| C | Potential energy, representing work done | 17,082,000 foot tons. Work of 1 horse, 719 days of 24 hours. | | 3,381,000 foot tons. Work of 1 horse, 142 days of 24 hours. | |

Energy has been defined as the power or capacity for doing work, and the work done in converting the simple elements of plant food, as represented in division A, into the complex organic compounds of division B, is at the expense of energy which is stored as an essential element of their constitution, and it may be approximately estimated as represented by the potential energy of these organic substances as given in division C of the table.

The larger expenditures of energy in exhalation by the plant and evaporation from the soil, of which quantitative estimates were given in my former paper, together with the heat required in warming the soil to maintain favorable conditions of growth, with losses by radiation from the extended surface of foliage, must all be included to represent the normal demands of the plant for energy which is derived from the heat and light of the sun.

The considerations presented in regard to the analysis of the corn will apply with equal force to the composition of the fat ox, which is compiled from the analysis made by Lawes and Gilbert, as reported in their valuable paper on the composition of animals. Here again, in the absence of better data, it has been assumed that the potential energy of the constituents of the ox (division C) represent the energy expended in their construction; but this is undoubtedly an underestimate of the real expenditures of energy in the constructive processes, as will be seen from an examination of further details.

The food of animals is usually said to consist of proteids, carbohydrates, and fats, with certain mineral constituents entering into the composition of plants or other animals on which they feed, but the significance of the potential energy of their food constituents, which represents the sole source of the energy expended by animals in their vital and mechanical activities, is quite generally overlooked, especially in popular discussions of the economy of foods.

On the ingestion of foods the first and most urgent demand of the organism

is for energy to be used in the constructive processes, and this must be obtained from the stored energy of the food itself. This stored energy may be liberated and transformed into heat by the disintegration of the organic substances in a variety of ways; as, for example, by the ordinary process of combustion, by the disintegrating action of microbes in fermentation and putrefaction, or by the destructive metabolism they undergo in the digestive organs of animals.

What are popularly represented as elements of nutrition must in fact be subjected to a destructive disintegration, and reduced almost to their original elements in the processes of digestion, in order to liberate their stored energy and make it available as the motive power of the animal economy. As stated in my former paper, but a comparatively small proportion of the so called food constituents is needed as new material for building tissues, and the larger portion is used as the passive agent in the transformation of energy.

The partly disintegrated food constituents in the alimentary canal, which are usually looked upon as waste materials, have, however, served a useful purpose by yielding supplies of their stored energy, which has a higher value, from the greater demand for it, than the chemical elements of their composition. The food constituents that have been more completely digested and retained by the animal to be used in tissue building have likewise contributed to the available supplies of energy in their destructive metabolism, and they may be represented as approximating the conditions presented in division A of the table. It is evident, as in the case of plants, that work must be done to transform these comparatively simple substances into the complex organic compounds of division B, and the destructive metabolism of food constituents that is required to supply the energy for the performance of this work must be recognized as important processes in the economy of nutrition.

In division C of the table the energy expended in the constructive processes of the animal is estimated from the potential energy of the organic substances enumerated in division B, but there are much larger expenditures of energy in collateral processes that must be taken into the account in order to determine the total demands for energy in the vital activities of the animal.

Every function of the animal economy involves the disintegration and reconstruction of tissues, or what is popularly called the wear and tear of the animal machinery, that necessitates a constant process of repair, so that the energy expended in the constructive processes alone, in the course of the life of the individual, from frequent repetition, would very much exceed the estimate that is based on the potential energy of the organism at any particular time.

Moreover, the energy expended in vaporizing water thrown off by the skin and lungs, considerably exceeds that used directly in tissue building, and losses also occur by radiation from the general surface.

The so called animal heat, which is regulated to enable the higher animals to maintain an independent and uniform temperature, is but the energy liberated in the destructive metabolism of foods and tissues, and it is not, as formerly assumed, the result of anything like a process of combustion.

From the constant and almost unlimited demands for energy as the motive power in every vital activity of animals, which must be derived directly or indirectly from the potential energy of their food, and the comparatively limited requirements for material elements in tissue building, it must be evident

that the chemical composition of foods, or the relative proportions of any of their proximate constituents, cannot serve as an index to their nutritive value.

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MANUAL LABOR AT AGRICULTURAL COLLEGES. By Prof. W. J. BEAL, State Agricultural College, Mich.

[ABSTRACT.]

COMPULSORY manual labor for wages has been tried more or less by many schools and colleges of this country, but by none, perhaps, has it had a longer and more thorough trial than at the Michigan Agricultural College. A provision requiring it was made in the State Constitution before the College was established, and since the first class entered in 1857, thirty-six years ago, the practice has been steadily maintained of requiring manual labor of the students for wages, twelve to fifteen hours per week. I must make this exception, viz.: since the establishment of a course in mechanical engineering, eight years ago, although students of that course have all been required to labor in the shops for eight hours per week, they have received no pay for the work, as it is all educational in its nature.

Since my first connection with the Michigan Agricultural College in 1870, I have uniformly been one of the most tenacious of the members of the Faculty to uphold in every way the practical or industrial side of the agricultural course, indoors and outdoors. For eleven years I had charge of Horticulture, as well as of Botany, and during that time half or more of the students were assigned to me for oversight of their work. During that period, and ever since, I have given a good deal of attention to the subject, and I assert that there has been no one thing at the College which has been the cause of so much trouble as the compulsory paid labor, — especially where students engage at ordinary work.

Twelve years ago I stated in the Rural New Yorker that, "Considering these difficulties, I believe the time will come when compulsory work for pay will be abandoned." I see no reason to change my opinion. With numerous trials by others who are earnest, ingenious, and persistent, the difficulties still remain.

I enumerate some of them: —

1. It is often very difficult to find profitable work at some seasons of the year.

2. The interruption of two short vacations in the growing seasons interferes with plans for continuous care of pieces of land.

3. Students *all* work at the same time of day, beginning at one o'clock P. M., and on this account there are often too many of them to work to advantage. They seldom complete jobs they begin, and thus lose interest.

4. As the numbers are large, there have very seldom been enough skilful foremen, so that students get into primitive methods of work, and form bad habits.

5. As the student receives pay he is expected to earn something, and this often leads the foreman to keep a man working at what he can do best, as is the custom in most factories, instead of frequently changing the kind of work, that the student may gain skill in many directions. Besides, he almost always prefers to work at what he can do best, thus insuring the highest rate of wages.

His sentiment is, "Put me anywhere that I may perform good work, then I shall get the highest wages."

6. Students are much more sensitive about receiving less than the maximum rate of wages, than they are in receiving low marks for recitations in the class. They often stoutly question the judgment of the foreman in placing an estimate on their labor, but usually acquiesce in the grade of marks given for recitations or examinations.

7. Too often he feels that he accomplishes little, and is working merely for the name of the thing.

Notwithstanding defects and difficulties of management, the present system of requiring labor as now carried on at the Michigan Agricultural College is far better than no manual training during the college course, but it has long seemed clear to me that it could be vastly improved.

One reason for requiring some manual labor in connection with a course of study is this. If a student performs no labor during his college course, he is not likely to return to labor when he leaves the college. The athlete who can jump, run, kick, vault, row, swim, skate, or throw a ball better than most of his associates and neighbors, delights in these sports, while the man who is unsuccessful in these things makes little effort, and never evinces much interest in his own efforts.

The young man who can harness a team properly, turn a straight and even furrow, shear a sheep quickly and to perfection, build a good haystack, mark out the land for corn, and run a cultivator so near the straight rows that scarcely a line is left uncultivated, will be proud of his achievements. He will economize time and strength, but make a success of his work.

So in the numerous details of work in horticulture, where such mechanical skill and alertness are required for excellence, while he who lacks training and success in manual operations dislikes the work wherein he displays that lack of skill. The same is true in the class-room and laboratory; if after a thorough training under the eye of a skilful teacher the young person becomes proficient in certain directions, he enjoys his studies.

We suppose a course in an agricultural college is to fit a man for farming in some of its numerous departments, but no matter how well he understands the theories of the subject and sciences pertaining thereto, he is not likely to engage in the business unless he also possesses a good knowledge of most kinds of manual operations pertaining to farming, and if he ventures to engage in the business without possessing this manual dexterity, he will have many a hard row to hoe, and very likely become discouraged, and sooner or later abandon the farm, because his training was unsymmetrical and incomplete.

To make the most of manual labor in an agricultural course, it should all or nearly all be performed with a view to acquiring skill, and not to the immediate returns. Skill in most farm and garden operations may usually be acquired in many directions if instruction is given and accompanied by practice. This practice is as much a necessary part of a thorough agricultural education as are the clinics for the young surgeon, or the practice with test tube and reagents for the chemist, or the correct use of a section cutter and reagents for the student of plant histology. The hand is trained with the eye and the mind, each helping the other, and without both the young person is not well qualified for success.

If a candidate is found already skilful in some kinds of work, I should by all means pass him accordingly, and not compel him to repeat the work as a condition of attendance. I should permit or even encourage him to acquire this skill elsewhere than at the college, during the vacations or before entering. In case young men have never engaged in farm work, I should insist that they acquire skill in certain operations common to farming before entering the college, or in vacations early in the course.

For example, every student before graduating should be at least fairly good at milking cows; but this simple operation need not be taught at the college, any more than arithmetic is taught there. So with numerous other sorts of labor. It is too much to expect a college course to include the teaching of all the elements of the handicraft of agriculture.

If students desire to work for pay and it can be supplied, let them work, and be rewarded according to their skill and faithfulness, but this should not be required.

You will conclude that my plan would be to exact of every student who enters the agricultural course, without exception, to pass a rigid examination in the various parts of farm and garden work, not expecting him to receive pay for work while learning how to perform it.

A course of manual training on farm or in garden cannot be so easily and pleasantly managed as a course of practice in shops for the degree of mechanical engineering, but it is practicable. In the shop, the student has a definite place to work, rain or shine, and is not exposed to mud or sunshine. The tasks can be more easily defined, and perhaps his success more easily gaged by some standard. It is needless to say that the examination for testing the proficiency in farm labor should not be oral nor in writing, but consist in actually performing many tasks assigned for the test.

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THE MATURING PACIFIC RAILROAD DEBTS. By RICHARD COLBURN. Elizabeth, N. J.

[ABSTRACT.]

THE present Congress ought to provide for payment of subsidy bonds issued 1865–69 to aid the pioneer railroad to the Pacific Ocean. As between the holders and the government they are a full obligation of the United States; and as they are security for circulating bank notes they can readily be extended at a low rate of interest, until it is convenient for the treasury to pay them.

But, as between the government and the railroad companies which received them, they are a debt to become then due and payable, together with arrears of unpaid interest amounting to 100 per cent of the principal, or an aggregate of $125,000,000. How is this to be managed? There will be a default in 1895, the mortgage security is utterly inadequate, but the carrying of the public mails, troops, and stores is a method of recovering some interest; if only the principal can be currently diminished the debt might finally be extinguished in the course of a century. This would be preferable to taking the roads and operating them by government agency, and for its account.

The figures will show that in the 25 years of operation, as a through line

about 80 per cent of interest has been repaid, and this may be safely taken as a basis of their ability to pay in the next 25 or 50 years, unless a great war by land or sea should break out. In view of the application of the Isthmus canals and other important improvements for national aid, the disposition of this case as a precedent is of moment. Too little should not be asked of these debtor corporations, both of which have forfeited claims to indulgence; but too much cannot be demanded without oppressing the local traffic or jeoparding the good condition of the road. A middle course may be found which will repay the advances of the government, and all the interest it will disburse, and which will gradually reduce the principal and cancel it in 100 years at farthest; to which may be added an inducement for the companies to redeem it in much less time, in case they prosper, without shutting off from the stockholders all hope of moderate dividends upon their investments.

Such a tabulated plan is submitted.

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RELATIONS OF PRODUCTION AND PRICE OF SILVER AND GOLD. By HENRY FARQUHAR, Coast Survey Office, Washington, D. C.

[ABSTRACT.]

EXHIBITION of a diagram showing (a) variations in annual production of silver, (b) ditto of gold, (c) ditto of relative value of the two. Account of some efforts to connect the last with the two preceding by a mathematical formula.

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THE NECESSITY FOR A BUREAU OF RECORD AND REFERENCE. By H. F. J. PORTER, Asst. Chief Dept. Machinery World's Columbian Ex., Chicago, Ill.

[ABSTRACT.]

THE ultimate object of scientific speculation and research is to obtain definite knowledge of facts which will serve as links in the chain of evidence necessary to formulate a definite theory of the creation. Unless all who are searchers in this large field are properly directed where to seek for information and instructed what to look for, and unless their discoveries are properly recorded for future reference, it is evident that much valuable time and labor will be lost, and that no definite results will be attained. If these premises are correct, there should be established a national repository where all the facts pertaining to this subject should be recorded. This repository should be in charge of a board of experts representing each of the great sciences, who would properly classify for record the information as it is received, and should direct the lines in which research should be made, and formulate a theory as far and as rapidly as the facts would justify. Astronomy, geology, botany, zoölogy, anthropology, and all other branches of science, should furnish evidence.

Such a bureau of record and reference should be established in Washington in connection with the Smithsonian Institute, and the American Association for the Advancement of Science, having representatives of all branches of science among its members, should take the initiative in promoting its formation.

GENEO-PATHOLOGICAL CHART. By LAURA OSBORNE TALBOTT, 927 P St., Washington, D. C.

[ABSTRACT.]

IT is well known how general is the custom among educated people to study genealogy, in order to ascertain the antecedents of the family relative to coats of arms and other heraldic devices worn by their ancestry, notwithstanding that these things had their origin in ignorance and barbarism.

Still there is a great fascination in these investigations, and our libraries are daily ransacked to find records bearing upon genealogical history.

Without interfering with this agreeable study, an additional course of investigation might attend it that might serve a purpose that would be of inestimable benefit to future generations.

This would consist in tracing out the pathological history of the family as far back and with as much detail as possible.

Both mental and physical traits, with accompanying diseases, could be brought together in the history of the individual and family upon a properly prepared chart, which from small beginnings might be of great value both to the family physician and to the psychologist.

The much needed information as to the constitution and inherited tendencies of the individual is of course extremely difficult to obtain in the present condition of science.

When a physician is called to a strange patient, not a little assistance would be rendered towards the formation of a correct diagnosis were there a pathological chart of the family at hand, whereby the physician could obtain a knowledge of the diseases current in the family for past generations.

Public opinion, no doubt, will have to undergo much change before such charts will become popular; but nevertheless, like cremation, co-operative housekeeping, and other conditions necessitated by new ways of living, I am sure that people of intelligence will come to appreciate the value of a pathological chart as well as the value of a genealogical tree.

In many families there are persons of studious habits, with leisure and means at command, who might follow out this line of work with as great detail as his or her ingenuity might desire, and reap their reward in the blessings of coming generations.

Questions might be formulated and sent to distant branches of the family in order to ascertain how many members of the family within the last half-century had succumbed to pulmonary or nervous diseases, what variety of disease, and in what condition of life and age.

Also how many cases of blindness from birth, or of deaf-mutism, had been known in the family, what intermarriages had taken place, and so on, with many details of questions if so desired. This is intended merely as a suggestion, but it is a subject that must interest every person of experience, for the reason that its purpose seems to be much farther reaching in being of possible service to future generations.

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THE UTILITY OF PRACTICAL PSYCHOLOGY. By LAURA O. TALBOTT, Washington, D. C.

# EXECUTIVE PROCEEDINGS.

## REPORT OF THE GENERAL SECRETARY.

The forty-second meeting of the American Association for the Advancement of Science was called to order on Thursday, August 17, 1893, at 10.20 A. M., in the Library Hall of the University of Wisconsin, Madison, Wis., by the retiring President, Professor Joseph LeConte of Berkeley, Cal. Professor LeConte said: —

There are two sciences that I love to compare with each other, namely, Astronomy and Geology, — the one the oldest of the sciences, the other among the youngest; the one dating back to the dawn of civilization, the other born with the present century. The domain of the one is infinite space, of the other is infinite time. The underlying law of the one is the universal law of gravitation, of the other the no less universal law of evolution. The one law is naught else than the Divine method of sustentation of the universe, the other the Divine process of creation of the universe. Geology to-day does homage to her elder sister Astronomy, and invests her with the mantle of authority in the person of her distinguished son, Dr. William Harkness, whom I now introduce to you as your President.

President Harkness arose and said: —

Ladies and Gentlemen, members and fellows of the American Association for the Advancement of Science, — I beg to assure you of my most profound appreciation of the compliment you have bestowed upon me by intrusting to my hands, for the ensuing year, the administration of the highest office in your gift. Fortunately for myself, the name and fame of the Association rests in the keeping of its fellows and members to a far greater extent than in that of any of its officers. Nevertheless, I should deem myself to have failed in my duty did I not most strenuously endeavor to maintain the high standard so successfully reached by my illustrious predecessor, Dr. LeConte. To aim at more would probably be to attempt the impossible; to be contented with less would be to sanction the loss of prestige already gained by the Association, and to that I do not believe any one here present would voluntarily consent.

The Rev. Joseph W. Cochran offered the invocation.

Mayor Corscot of Madison, in a few appropriate remarks, then extended, on behalf of the municipal authorities, a warm welcome to the Association.

General Lucius Fairchild, President of the Local Committee, extended also a most cordial welcome.

Dr. CHARLES K. ADAMS, President of the University of Wisconsin, eloquently welcomed the Association to the University.

President HARKNESS, in behalf of the Association, thanked the several speakers for the welcome so earnestly extended to its members, and explained briefly how the Association endeavored to advance science, and how its methods compared with those of the great European scientific societies.

The GENERAL SECRETARY announced that the COUNCIL recommended that the daily sessions be held on Thursday, Friday, Monday, and Tuesday, from 10 to 12 o'clock A. M., and from 2 to 5 o'clock P. M. Upon motion this recommendation was adopted.

The GENERAL SECRETARY announced that in view of the fact that many members wished to attend the Scientific Congresses held in connection with the Columbian Exposition and beginning on August 21, the COUNCIL approved of sectional meetings being held outside of the scheduled hours.

He also announced that a lecture complimentary to the citizens of Madison would be delivered by Dr. D. G. BRINTON on Friday evening in the Assembly Room of the Capitol, and that the meeting of the Nominating Committee would be held immediately after the lecture, in the Supreme Court Room.

He also stated that 84 new members had been elected since the last meeting, and that thus far 113 papers had been received for the present meeting. It was requested that all nominations for fellowships should be presented to the GENERAL SECRETARY before Friday evening.

The PERMANENT SECRETARY read the list of deceased members notices of whose death have been received since the last meeting. As he read the names, Professor PUTNAM made remarks upon those who had been more or less prominent in connection with the Association. He dwelt particularly upon the names of Dr. J. S. NEWBERRY of New York, who was elected at the fifth meeting of the Association, was President of the sixteenth meeting (Burlington), and was 76 years old at the time of his death; Prof. EBEN NORTON HORSFORD of Cambridge, who was an original member of the Association, and its General Secretary for the second meeting, and was aged 75 at the time of his death, Jan. 1, 1893; Dr. HENRY WHEATLAND of Salem, aged 81, one of the founders of the Essex Institute of that city, who was an original member of the Association, and for many years an auditor; Dr. P. R. HOY of Racine, Wis., aged 76, a member from the seventeenth meeting; Mrs. MARTHA J. LAMB, aged 64, late editor of the "Magazine of American History"; and Mrs. MARY B. ALLEN KING, of Rochester, N. Y., a member since the fifteenth meeting, who for several years has been the oldest member of the Association and died at the age of 94.

THOMAS ANTISELL, Washington, D. C. (33). Died 1893.

CHARLES S. CLARKE, Peoria, Ill. (34). Died Nov. 15, 1890.

G. W. COAKLEY, New York. Died August, 1893.

E. MIRIAM COYRIÈRE PARDO, New York (36). Born in London, England, Sept. 2, 1845. Died in New York, Feb. 6, 1893.

I. THOMAS DAVIS, Washington, D. C. (40). Died Jan. 19, 1892.

FRED JAMES DORAND, Chester, Vt. (38). Born in Rockingham, Vt., Dec. 6, 1856. Died in Aiken, S. C., April 17, 1893.

JOHN MELMOTH DOW, New York, N. Y. (31). Died in New York, Nov. 4, 1892.

JOHN W. DOWLING, New York, N. Y. (36). Born in New York, Aug. 15, 1837. Died in Goshen, N. Y., Jan. 15, 1892.

CHARLES DROWNE, Canaan Four Corners, N. Y. (6). Died in 1888.

MOSES G. FARMER, Eliot, Me. (9). Born in 1820. Died in Chicago, Ill., May 25, 1893.

E. T. FRISTOE, Washington, D. C. (40). Died 1892.

FRIEDRICH AUGUSTUS GENTH, Philadelphia, Pa. (24). Honorary Fellow, 1888. Born in Waechtersbach, Hesse Cassel, May 17, 1820. Died in Philadelphia, Pa., Feb., 2, 1892.

WILLIAM J. GORDON, Cleveland, Ohio (29). Died Nov. 23, 1892.

CHARLES W. HASTINGS, Kansas City, Mo. (38). Died in Brooklyn, N. Y., Oct. 24, 1892.

J. E. HENDRICKS, Des Moines, Iowa (29). Died June 8, 1893.

THOMAS HOCKLEY, Philadelphia, Pa. (33). Died March 12, 1892.

JOHN J. HOGSETT, Danville, Ky. (39). Died Jan. 18, 1891.

H. D. KENDALL, Grand Rapids, Mich. (35). Died in Guaryınas, Mexico, Jan. 28, 1891.

WILLIAM LEE, Washington, D. C. (29). Died March 2, 1893.

MRS. JOHN LUCAS, Philadelphia, Pa. (33). Died May 8, 1893.

ISAAC C. MARTINDALE, Camden, N. J. (26). Died Jan. 3, 1893.

CHARLES NETTLETON, New York, N. Y. (30). Born in Washington, Conn., Oct. 2, 1819. Died in New York, May 5, 1892.

LEWIS M. NORTON, Boston, Mass. (29).

WILLIAM P. SEYMOUR, Troy, N. Y. (19). Died April 7, 1893.

GEORGE VASEY, Washington, D. C. (32). Died in Washington, March 4, 1893.

ALICE W. WHEILDON, Concord, Mass. (31). Died June 16, 1893.

The PERMANENT SECRETARY then read his financial report for the year ending Aug. 1, 1893.

The LOCAL SECRETARY announced that a daily lunch, at small cost, would be served in the basement of the Science Hall of the University during the week of the Association. Members were informed that certificates of the payment of railroad fares to Madison must be presented to the Local Secretary for indorsement, and details were given with regard to the various excursions announced for Saturday and following days. The Session then adjourned.

The Library Hall at this and all subsequent sessions of the Association, was handsomely decorated by festoons of evergreen and by banners. Each day fresh flowers were placed by the ladies of Madison on the tables in Library Hall and in the rooms assigned to the different sections.

SAME DAY, 8 P. M. GENERAL SESSION convened in the Assembly Chamber of the Capitol, President HARKNESS in the chair.

In the absence of Governor PECK of Wisconsin and at his request, the ATTORNEY GENERAL welcomed the Association on behalf of the State authorities in an earnest address, in which he spoke of the growth and strength of Wisconsin's educational system, supported through two distinct sources, — through grants of land made by the General Government to this State, and by the direct taxation of the people. He said: —

"During the year 1882 the total tax levied and collected in this State for the support of our school system was, in round numbers, for the common schools

$2,000,000, State University $75,000, normal schools $346,000, making a total for that year of $2,111,000. In 1892, ten years later, the total tax levied and collected for school purposes was in round numbers as follows: For the common schools, $4,000,000; free high schools, $50,000; normal schools, $68,000; State University, $180,000, or a total of $4,298,000, leaving an increase of an annual tax of $2,186,000 to represent the growth in ten years of the sentiment of our people in favor of popular education. A comparison of any other set of years will show an equally favorable result. Step by step our educational system has developed. Step by step it is graded from the common school to the high school, from the high school to the crown jewel of our system, the pride and the glory of our people, the Wisconsin State University."

The retiring President, Professor JOSEPH LECONTE, then delivered his annual address to a large and attentive audience. (The address is printed in full in this volume.)

After the adjournment of the Session, an informal reception in the Senate Chamber of the Capitol, was tendered the Association by the citizens of Madison. The attendance was large and the occasion was thoroughly enjoyable.

FRIDAY, AUGUST 18, 1892. GENERAL SESSION in the Library Hall of the University of Wisconsin, at 10 A. M. President HARKNESS in the chair.

The GENERAL SECRETARY announced the meeting of the Committee on Fellowships in the Capitol, at 7 P. M., and the meeting of the Nominating Committee in the Supreme Court Room at the close of the Friday evening lecture. He announced the total number of new members elected as 94, and the total number of papers presented thus far for this meeting as 148.

The LOCAL SECRETARY stated that all registration for the excursions announced must be made before Friday evening. Also that the Association Post-office would be open 7.30 to 8.30 P. M. on Saturday, and 3 to 4 P. M. on Sunday.

The Session then adjourned.

SAME DAY, 8 P. M. A GENERAL SESSION was called to order in the Assembly Chamber of the Capitol by President HARKNESS. He introduced Dr. D. G. BRINTON of Media, Pa., who delivered a lecture, complimentary to the citizens of Madison, "On the Earliest Men." The Session then adjourned.

On Friday afternoon, from 4 to 6, Mrs. C. K. ADAMS, assisted by the wives of the Faculty of the University of Wisconsin, received the ladies in attendance at this meeting of the Association. The reception was largely attended, and the occasion was one of great pleasure.

SATURDAY, AUGUST 19, 1893. The day was given up to the two following excursions by special trains: —

To Devil's Lake, leaving at 8.45 A. M.

To the "Dells of the Wisconsin," leaving at 8 A. M.

The features of interest connected with the fauna, flora, and geology of both the regions visited are fully described in the special circular of the Local Committee. The geologists participating in the excursion to Devil's Lake were conducted by Prof. C. R. Van Hise; those who went to the "Dells" were guided by Prof. T. C. Chamberlin and Prof. R. D. Salisbury. Both excursions were largely attended. They were in every respect successful, and were highly appreciated and enjoyed by the members of the Association.

MONDAY, AUGUST 21, 1893. The Association was called to order in GENERAL SESSION at 10 o'clock A. M., in Library Hall, by President HARKNESS.

The LOCAL SECRETARY announced the hours for the shorter excursions in the vicinity arranged for different sections.

The GENERAL SECRETARY announced that the NOMINATING COMMITTEE recommended that the next meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE begin on the third Thursday in August, 1894, and that the PRESIDENT and PERMANENT SECRETARY of the Association be appointed a committee with power to decide upon the place of meeting. The recommendation was unanimously adopted.

The following fellows were recommended by the NOMINATING COMMITTEE as officers for the next meeting: —

## PRESIDENT.

DANIEL G. BRINTON, Media, Pa.

## VICE-PRESIDENTS.

A. **Mathematics and Astronomy.** — GEO. C. COMSTOCK, Madison, Wis.
B. **Physics.** — WILLIAM A. ROGERS, Waterville, Me.
C. **Chemistry.** — THOMAS H. NORTON, Cincinnati, Ohio.
D. **Mechanical Science and Engineering.** — MANSFIELD MERRIMAN, South Bethlehem, Pa.
E. **Geology and Geography.** — SAMUEL CALVIN, Iowa City, Iowa.
F. **Zoology.** — SAMUEL H. SCUDDER, Cambridge, Mass.
G. **Botany.** — LUCIEN M. UNDERWOOD, Greencastle, Ind.
H. **Anthropology.** — FRANZ BOAS, Worcester, Mass.
I. **Economic Science and Statistics.** — HENRY FARQUHAR, Washington.

## PERMANENT SECRETARY.

F W. PUTNAM, Cambridge (office Salem), Mass. (Holds over.)

## GENERAL SECRETARY.

H. L. FAIRCHILD, Rochester, N. Y.

## SECRETARY OF THE COUNCIL.

JAS. LEWIS HOWE, Louisville, Ky.

## SECRETARIES OF THE SECTIONS.

A. **Mathematics and Astronomy.** — WOOSTER W. BEMAN, Ann Arbor.
B. **Physics.** — BENJ. W. SNOW, Madison, Wis.
C. **Chemistry.** — S. M. BABCOCK, Madison, Wis.
D. **Mechanical Science and Engineering.** — JOHN H. KINEALY, St. Louis.
E. **Geology and Geography.** — WM. MORRIS DAVIS, Cambridge, Mass.
F. **Zoology.** — WM. LIBBEY, JR., Princeton, N. J.
G. **Botany.** — CHARLES R. BARNES, Madison, Wis.
H. **Anthropology.** — ALEXANDER F. CHAMBERLAIN, Worcester, Mass.
I. **Economic Science and Statistics.** — MANLY MILES, Lansing, Mich.

## TREASURER.

WILLIAM LILLY, Mauch Chunk, Pa.

The Secretary was unanimously authorized to cast the ballot of the Association for the election of the fellows nominated. The President declared them duly elected.

The GENERAL SECRETARY announced that the number of new members elected at this meeting thus far was 97, and that the number of papers presented was 169.

He also announced that, in response to a united recommendation of Sections F and G, the COUNCIL had appropriated $100 from the income of the Research Fund for a table at the Marine Biological Laboratory at Wood's Holl for the season of 1894; and further that the President had named the following committee of five to make the appointment to the table: Dr. SAMUEL H. SCUDDER (chairman), Prof. CHARLES E. BESSEY, Prof. LUCIEN M. UNDERWOOD, Prof. HENRY F. OSBORN, Dr. L. O. HOWARD.

The GENERAL SECRETARY read the following telegram, sent at the request of the COUNCIL to the veteran physicist Professor VON HELMHOLTZ, who has just arrived at the World's Congress of Physicists at Chicago:—

The American Association for the Advancement of Science, in session at Madison, Wisconsin, sends to Professor Von Helmholtz greeting and welcome.

WM. HARKNESS, *President of the Association.*

He also read the telegraphic reply to the same:—

*Chicago, August 21, 1893.*

TO AMERICAN ASSOCIATION ADVANCEMENT OF SCIENCE,

Accept best thanks for friendly greeting received to-day.

HERMANN VON HELMHOLTZ.

The GENERAL SECRETARY read the following letters:—

*Chancellor's Office, Vanderbilt University,*
*Nashville, Tenn., August 16, 1893.*

TO THE COUNCIL OF THE
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

GENTLEMEN:

Appreciating very highly the work of your Association, and in full sympathy with the purposes and aims of the same, I beg to extend to you, in the name of the authorities of Vanderbilt University, a cordial invitation to hold your annual meeting in 1896 on our Campus, and we hereby tender the use of our halls and grounds for this purpose.

Very respectfully,
J. H. KIRKLAND, *Chancellor.*

*City of Nashville, Office of the Mayor,*
*Nashville, Tenn., August 17, 1893.*

TO THE COUNCIL OF THE
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE,
IN SESSION AT MADISON, WISCONSIN.

GENTLEMEN:

Being informed that it is the intention of the authorities of Vanderbilt University of this city to extend an invitation to your Association to hold its annual

meeting in 1896 at the University, and being desirous of expressing the appreciation felt by myself and colleagues of the magnificent work of your Association and of its objects, I take great pleasure in supplementing this invitation, and beg most sincerely to express the hope that it may be the decision of your Council to decide to hold the annual meeting of the Association in 1896 within our city, for which occasion I can assure you of the sincere welcome of our citizens which is sure to await you.

Again assuring you of the pleasure our citizens will feel in receiving your Association, and our interest and sympathy in your work,

I am, gentlemen,

Very sincerely yours,

GEO. B. GUILD, *Mayor*.

The GENERAL SECRETARY announced that the following Members had been elected Fellows by the COUNCIL, on Aug. 21, 1893: —

Cushing, Frank H., Bureau of Ethnology, Washington, D. C. (40). **H**
Darton, Nelson H., U. S. Geol. Survey, Washington, D. C. (37). **E**
Gillette, C. P., Prof. Zool. and Entom., Colorado Agr. College. **F**
Glenn, William, 1348 Block St., Baltimore, Md. (33). **C**
Grant, Ulysses S., Geol. Survey of Minnesota, Minneapolis, Minn. (39). **F**
Griswold, Leon Stacy, 238 Boston St., Dorchester, Mass. (38). **E**
Gurley, William F. E., Danville, Vermilion Co., Ill. (37). **E**
Hallock, Dr. William, Columbia College, New York, N. Y. (40). **B E**
Harris, Gilbert D., Asst. Paleont., U. S. Geol. Survey, Smithsonian Institution, Washington, D. C. (37).
Hathaway, Prof. A. S., Rose Polytechnic Institute, Terre Haute, Ind. (41). **A**
Hayes, Charles Willard, U. S. Geol. Survey, Washington, D. C. (44). **E**
Hobbs, William Herbert, Ph. D., Asst. Prof. Min. and Petrol., Madison, Wis. (41). **E**
Hubbard, Gardiner Green, 1328 Connecticut Av., Washington, D. C. (40). **E**
Huston, Henry A., LaFayette, Ind. (27). **C**
Jones, Prof. Marcus E., Salt Lake City, Utah (40). **E**
Kellerman, Prof. William A., Ohio University, Columbus, Ohio (41). **F**
Knorr, Aug. E., 1109 14th St., N. W., Washington, D. C. (40). **C**
Knowlton, Frank H., Dep't of Botany, U. S. National Museum, Washington, D. C. (33). **F**
Lacoe, R. D., Pittston, Pa. (31). **E F**
McDonnell, Prof. Henry B., College Park, Md. (40). **C**
Mark, Prof. E. H., Louisville, Ky. (39). **B**
Marsh, Prof. C. Dwight, Ripon, Wis. (34). **F E**
Mercer, H. C., Doylestown, Bucks Co., Pa. (41).
Nagle, James, C., A. & M. College Station, Texas (40). **D B**
Newell, F. H., U. S. Geol. Survey, Washington, D. C. (40).
Newell, William Wells, Sec'y Am. Folk Lore Society and Editor of the Journal, 175 Brattle St., Cambridge, Mass. (41). **H**
Nichols, Ernest Fox, Hamilton, N. Y. (41). **B**
Orndorff, Dr. William Ridgely, Cornell University, Ithaca, N. Y. (41). **C**

Reid, Prof. Henry F., Case School Applied Science, Cleveland, Ohio (36). B
Ritter, W. F. McK., P. O. Box 50, Milton, Pa. (40).
Robinson, Benjamin Lincoln, Curator Harvard Herbarium, Cambridge, Mass. (41). F
Skinner, Aaron N., U. S. Naval Observ., Washington, D. C. (40). A
Stevenson, Mrs. Matilda C., Bureau of Ethnology, Washington, D. C. (41). H
Thompson, Joseph Osgood, Haverford College, Pa. (41).
Waite, M. B., Department of Agriculture, Washington, D. C. (37).
Weeks, Joseph D., Editor American Manufacturer, Pittsburgh, Pa. (35). D
Winterhalter, A. G., Lt. U. S. N., care Navy Dept., Washington, D. C. (37). A

The Session then adjourned.

In the afternoon there was an excursion, intended more especially for anthropologists, by steamer across Lake Mendota to the extensive group of effigy mounds on the borders of the lake.

In the evening, from 8 to 11, a brilliant lawn fête was given in honor of the Association by the LOCAL COMMITTEE on the extensive grounds adjoining the residences of President C. K. Adams, Mr. George Raynor, and Mr. John Olin, and facing on Lake Mendota. These lawns were handsomely illuminated by colored electric lights and by Chinese lanterns. Governor Peck of Wisconsin and Mrs. Peck welcomed the visitors beneath a canopy, while in a neighboring pavilion refreshments were served. An orchestra was stationed in a small steamer which moved about the lake in the vicinity. Other steamers took out small parties at frequent intervals to view the gay scene from the water, and to observe the general illumination of the grounds along the lake shore, extending to a distance of nearly a mile. About one thousand persons were present, and all carried away the pleasantest remembrances of the occasion, which in its brilliancy and fairy-like effects stands alone among the many receptions tendered the Association.

TUESDAY, AUGUST 22, 1893. The Association was called to order in GENERAL SESSION, at 10 A. M., in Library Hall, by President HARKNESS.

The GENERAL SECRETARY announced that 98 new members had been elected, and read a letter from Professor Elisha Gray, inviting members of the Association to participate in a grand banquet to be given in honor of Professor von Helmholtz and other foreign official delegates to the World's Electrical Congress, to be held in Chicago on Thursday evening, August 24.

The GENERAL SECRETARY read the following resolution from Section G, which was favorably recommended by the COUNCIL: —

*Whereas*, At the request of the officers of Section G, material has been accumulated during the past year in reference to the present status of botanical laboratories and instruction in American universities and colleges, and a preliminary paper of educational importance has been presented to the Section on this subject; and

*Whereas*, It is the opinion of the members of Section G that the publication of the full statistics on which this paper is based would be of great importance in the advancement of botanical instruction; therefore

*Resolved*, That the United States Commissioner of Education be requested to consider the matter of publishing this information in the form of a Bulletin of the Bureau of Education, under the authorship of Professor Conway Macmillan, by whom the statistics have been collected.

It was unanimously voted to approve of the request.

The Session adjourned.

During the afternoon an excursion for the benefit of Section E (Geology) was made by carriage into the driftless area west and southwest of Madison.

An excursion for the benefit of Section G (Botany) was also made by carriage to Lake Wingra, where opportunity was afforded to study lake, swamp, and hill flora.

SAME DAY, 8 O'CLOCK P. M. The Association was convened in the Assembly Chamber of the Capitol. PRESIDENT HARKNESS in the Chair.

The COUNCIL recommended that the Committee of an Honorary Agent of Transportation be discontinued. Adopted.

The Committee to apply to Congress for a reduction of the Tariff on Scientific Books and Apparatus reported progress. The report was accepted and the Committee continued.

The Committee to Memorialize Congress to take steps for the preservation of Archæologic Monuments on the Public Lands reported progress. The report was accepted and the Committee continued.

The Committee on Water Analysis reported progress. The report was accepted and the Committee was continued.

The Committee on the Maintenance of Timberlands and on the Development of the Natural Resources of the Country reported progress. The report was accepted and the Committee continued.

The Committee on Indexing Chemical Literature reported. The report was received and the Committee continued. In accordance with a recommendation from Section C it was voted that the resignation of Prof. A. A. Julien from this Committee be accepted and Dr. H. W. WILEY be appointed in his place.

The Committee to secure an American Table at the International Marine Biological Station, Naples, Italy, presented no report. On recommendation of the Council it was voted that the Committee be discontinued.

The Committee on Standards for Astronomical and Physical Units presented no report. On recommendation of the Council it was voted that the Committee be discontinued.

The Committee on the Endowment of Research Fund reported progress. It was voted to continue the Committee, and to add to its number Prof. N. L. Britton, of New York, from Section G.

The Committee on a Table at the proposed Marine Biological Station at Jamaica reported that no progress had been made, as nothing had been done toward establishing the station. On recommendation of the Council it was voted to discontinue the Committee.

The Committee on the Preservation of the Ancient Earthworks near Anderson, Indiana, presented no report. On recommendation of the Council it was voted to discontinue the Committee.

Report of the Committee on Biological Nomenclature. — The Committee's Report presented at the Rochester meeting was printed and widely distributed among biologists, and sent to many scientific periodicals. It was commented on by various of the periodicals to which it was sent, and translated and printed entire in the "Biologisches Centralblatt." There seems to be considerable interest in the subject at the present time; hence the Committee ask that this be accepted as a report of progress, and that the Committee be continued.

S. H. GAGE, *for the Committee.*

The report of the Committee was received, and the Committee was continued.

The Committee to confer with the Director General of the World's Columbian Exposition to secure special headquarters during the Exposition for the Sciences represented by the nine sections of the Association, and the Committee to co-operate with the World's Congress Auxiliary of the World's Columbian Exposition for the holding of International Scientific Congresses during the Exposition reported on what had been accomplished in their different fields; and as there was no longer any necessity for their existence, it was voted, on recommendation of the COUNCIL, that the two Committees be discontinued.

On recommendation of the COUNCIL it was voted —

That the final report of the Standing Committee of the Association on "Spelling and Pronunciation of Chemical Terms," presented at the Washington meeting of the Association (1891) and published in the Proceedings of that meeting, be adopted, and that the Committee be discharged in view of the fact that the action of the Association at that time was omitted from the printed record.

The following resolution from Section E, approved by the COUNCIL, was adopted by the Association: —

*Whereas,* The accurate knowledge of the geological range and distribution of fossils is known to be of extreme value in classifying and correlating geological formations; and

*Whereas,* The specialization of scientific studies has led to the separation to a considerable extent of the paleontologists from the Geological section and their attraction into the sections of Zoology and Botany; and

*Whereas,* This separation has greatly interfered with, and to a considerable degree prevented, the intelligent discussion of important questions of mutual interest to both geologists and biologists:

*Therefore,* to promote the interests of geological science, Section E recommends the appointment of a standing committee of five members whose business it shall be to consider and report upon such questions, and to encourage and organize investigations of problems of geological, as distinguished from purely biological paleontology. This committee shall have power to add to its number, and shall be known as the Standing Committee of the Association on Recording and Classifying Fossil Faunas and Floras.

The following committee was appointed: H. S. WILLIAMS, C. D. WALCOTT, H. F. OSBORNE, S. H. SCUDDER, and ARTHUR HOLLICH.

The following resolution, presented by Section G and approved by the COUNCIL, was adopted: —

*Resolved,* that Section G requests the appointment of a Standing Committee on Instruction in Botany in the secondary schools of the country.

On the nomination of the Section the following fellows were appointed as the Committee: — Dr. J. M. COULTER, Lake Forest, Ill.; Dr. D. H. CAMPBELL, Palo Alto, Cal.; and Dr. N. L. BRITTON, New York.

The following resolution, presented by Section H and approved by the COUNCIL, was voted: —

*Whereas*, The Pacific Forest Reserve in the State of Washington, including all of Mount Ranier and comprising an area of 36 by 42 miles, or 987,580 acres, has been set apart by Presidential proclamation; and

*Whereas*, This Reservation is of exceptional scientific interest by reason of its magnificent glaciers, its remarkable volcanic products and residual volcanic activity, its abundant flora and fauna whose characters are dependent with singular directness on climate and other environmental conditions; and

*Whereas*, No provision has been made for the protection and preservation of this Reserve, and its forests and game are already invaded and injured by irresponsible parties: Therefore

*Resolved*, That a committee of five members or fellows of the A. A. A. S. be appointed to memorialize Congress to make early provision for the protection and preservation of the Pacific Forest Reserve of the State of Washington by making it a National Park.

The following Fellows were chosen members of this Committee: Major J. W. POWELL, Prof. JOSEPH LECONTE, Prof. I. C. RUSSELL, Dr. B. E. FERNOW, and Dr. C. H. MERRIAM.

The PERMANENT SECRETARY presented the following statistics of the meeting: —

The Retiring President and the nine Vice Presidents had delivered addresses. A Lecture complimentary to the citizens of Madison had been given. One hundred and sixty-eight papers had been read in the sections as follows: Section A 9, B 20, C 18, D 8, E 23, F 13, G 34, H 35, I 8, = 168. Ninety-seven new members had been elected, and thirty-seven members had been made fellows.

The members and associates in attendance had come from: Madison 22, other places in Wisconsin 9, New York 38, District of Columbia 30, Ohio 25, Illinois 20, Massachusetts 19, Indiana 13, Iowa 12, Pennsylvania 11, Michigan 9, Missouri 8, Maryland 7, New Jersey 5, Connecticut 5, California 4, Kansas 4, Tennessee 3, West Virginia 3, North Carolina 3, Minnesota 3, Colorado 3, Iowa 3, Florida 2, Wyoming 2, South Dakota 2, Kentucky 2, Georgia 2, Mississippi 2, Virginia 1, Nebraska 1, New Mexico 1, Rhode Island 1, Maine 1, North Dakota 1, South Carolina 1, Texas 1, Canada 8, Germany 3, = 290.

DR. D. G. BRINTON on behalf of the COMMITTEE ON RESOLUTIONS, presented the following: —

*Whereas*, The meeting of the American Association for the Advancement of Science held in Madison has been noteworthy for its agreeable and successful continuance; and

*Whereas*, This satisfactory result has been largely due to the courtesy, energetic attentions, and liberality of certain institutions and individuals of the City and State:

*Therefore*, It is hereby resolved that the cordial and earnest thanks of the Association be tendered, —

1. To the State of Wisconsin and to its chief Executive officer, Governor

G. W. PECK, for the use of halls in the Capitol Building and the supply of tents for the reception on the lawn.

2. To the City of Madison, its citizens and its Mayor, Mr. JOHN CORSCOT, for the labor they have expended in preparations for the meeting, the funds they have contributed to its expenses, and for the genial hospitality extended to the members.

3. To the State Historical Society at Wisconsin, and its secretary, Mr. R. G. THWAITES, for their assistance in preparations for the meeting.

4. To the WISCONSIN ACADEMY OF SCIENCES, ARTS, AND LETTERS, for their contribution to the expenses of the meeting.

5. To the STATE UNIVERSITY OF WISCONSIN, its regents and its president, for the admirable facilities extended to the Association in the use of its halls and buildings, and for the liberal appropriation voted for the expenses of the meeting.

6. To the LADIES of Madison for their graceful hospitality to the members of the Association and their families, and for the floral decorations of the halls occupied by the Association and its sections.

7. To the LOCAL COMMITTEE OF ARRANGEMENTS, its president, General LUCIUS FAIRCHILD, its secretary, Prof. E. R. BARNES, and to all its members, for the efficiency and enterprise with which they have fulfilled their onerous duty, and secured to the Association one of its most agreeable and interesting meetings.

8. To the COMMITTEE ON EXCURSIONS, who have furnished members of the Association a series of highly appreciated opportunities to visit points of interest in the State, and to learn by personal inspection the beauty of its scenery.

9. To the RAILWAY COMPANY of the city and to the PRESS, which have offered facilities for the transportation of members and the report of our proceedings.

These resolutions were seconded by Prof. JOSEPH LECONTE, Prof. WILLIAM H. BREWER, Rev. H. C. HOVEY, Mr. WARREN K. MOOREHEAD, Mr. W. J. MCGEE, Rev. J. OWEN DORSEY, Prof. CHARLES E. BESSEY, and Mrs. NELLIE S. KEDZIE, who all expressed in felicitous terms their appreciation of the many courtesies received during the Madison meeting.

The resolutions were unanimously adopted.

President HARKNESS then remarked upon the character of the meeting, and after congratulating the members upon all that had been accomplished, and the many pleasant features of the meeting, pronounced the forty-second meeting of the American Association for the Advancement of Science adjourned *sine die.*

THOMAS H. NORTON,
*General Secretary.*

## REPORT OF THE PERMANENT SECRETARY.

When it was determined to hold the meeting of 1893 in the vicinity of Chicago, it was thought that the meeting would receive large accessions of members, and particularly of foreign scientists who were gathered at Chicago during the World's Columbian Exposition; and also that members could more conveniently attend if the meeting was held near the great attraction of the year. This expectation was not realized, owing to the unprecedented success of the Exposition, and the many wonderful attractions of the "White City" which kept fast hold of all who once entered its gates. There is no doubt, also, that the long series of congresses, including many especially devoted to departments of science included in the sections of the Association, drew a large number of our members to Chicago at the very time of the Madison meeting. The meeting was therefore not so largely attended as was anticipated, although it was a remarkably pleasant one in every particular. The University buildings furnished admirable accommodation for the Sections, General Sessions, and offices; while the Assembly Chamber in the Capitol was a central and comfortable hall for the larger evening gatherings. All the arrangements of the Local Committee were well planned, and were carried out with efficiency and with careful thought for the comfort of the members and the success of the meeting. The social events and excursions were most charming, and certainly the beautiful lawn fête was a worthy rival to the evening illuminations of the "White City."

Those who left the World's Fair to attend the meeting were well satisfied with the restful days at Madison, filled as they were with all that makes life intellectually enjoyable.

The experiment of beginning the meeting with the session of the Council on Wednesday, and the first General Session on Thursday, was regarded as successful, and determined the continuance of the plan for the next meeting. It certainly has many advantages, and so far as can be decided by one meeting, no disadvantages.

Several of the affiliated societies held their meetings just preceding or following the Association week, but others were affected by the congresses held in Chicago.

The statistics of the Madison meeting are given in the preceding report of the General Secretary. The general statistics of the Association are as follows:—

Of the 98 members elected since the Rochester and during the Madison meeting, 3 have declined membership, 1 was already a member, and 59 have perfected their membership, as have 7 who were elected at the Rochester meeting; 32 members have paid their arrears and these have been restored to the roll.

This makes 98 names added to the roll since the Rochester volume was published.

From the Rochester list 39 names have been transferred to the list of deceased members, among them 2 founders, 1 patron, and 1 honorary fellow; 23 members and fellows have resigned, and 134 have been omitted for arrearages, making a deduction of 196 from the list.

4 members have become life members and 46 members have been transferred to the roll of fellows.

The following is a comparative statement of the roll as printed in the Washington and Rochester volumes and in the present volume: —

| | Washington. | Rochester. | Madison. |
|---|---|---|---|
| Patrons . . . . . . . | 3 | 3 | 2 |
| Corresponding members . . | 3 | 2 | 2 |
| Members . . . . . . . | 1,269 | 1,246 | 1,138 |
| Honorary Fellows . . . . | 2 | 2 | 1 |
| Fellows . . . . . . . | 777 | 784 | 796 |
| | 2,054 | 2,037 | 1,939 |

The distribution of publications since the last report is as follows: —

Memoirs No. 1: sold 2 copies, exchange 1 copy. Transactions: sold 2 copies, exchange 1 copy. Proceedings, Vols. 1–40: delivered to members, 183; sold, 159; exchanges, 82; duplicate copies to members, 15; bought 25 copies; received as donations, 83; returned from members not found, 4; presented, 23; returned from exchanges, 5.

Vol. 41: delivered to members, 1,580; sold, 24; exchanges, 239; presented, 11; duplicate copy to member, 1; returned from members not found, 2; returned from exchanges, 2.

Vol. 42: subscription paid for 1 copy.

The cash account following this shows a balance of cash on hand on current account of $1875.46.

The invested funds of the Association are as follows:

| | | |
|---|---|---|
| Aug. 1, 1892. RESEARCH FUND . . . . | $5404.27 | |
| Grants paid . . . . . . . . . . | 200.00 | |
| | $5204.27 | |
| Aug. 1, 1893. Interest at 5% . . . . . . . | 260.21 | |
| " " Life Commutations added . . | 100.00 | |
| | $5564.48 | |
| Grant made at Madison Meeting for table Woods Holl Laboratory . . . . . . . . | 100.00 | |
| | $5464.48 | |
| Aug. 1, 1893. GENERAL FUND Principal and interest . . . . . . . | 136.29 | $5600.77 |

F. W. PUTNAM,
*Permanent Secretary.*

*Salem,* April 30, 1894.

# CASH ACCOUNT

OF THE

# PERMANENT SECRETARY.

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

| | | |
|---|---|---|
| 1892–93. | | |
| To balance from last account . . . . . . . . . . | | $2,394 53 |
| Admission fees Rochester meeting . . . . . . . | $640 00 | |
| " " previous to Rochester meeting . . | 20 00 | |
| Fellowship fees . . . . . . . . . . . . . . . | 102 00 | |
| | | 762 00 |
| Assessments previous to Rochester meeting . . . | 717 00 | |
| " for Rochester meeting . . . . . . . | 3,438 00 | |
| " " Madison meeting . . . . . . . . | 372 00 | |
| Associate fees for Rochester meeting . . . . . | 156 00 | |
| | | 4,683 00 |
| Publications sold . . . . . . . . . . . . . . | 170 09 | |
| Received for binding . . . . . . . . . . . . | 116 55 | |
| " " express, postage, etc. . . . . . . . | 7 20 | |
| | | 293 84 |
| Life members' commutation . . . . . . . . . . | 100 00 | |
| From income Research Fund . . . . . . . . . | 200 00 | |
| | | 300 00 |
| | | $8,433 37 |

I have examined the above account, and

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| | | 1892–93. |
|---|---|---|
| By 2,500 copies Proceedings, Vol. 41 | | |
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| | | 315 19 |
| Expenses of Section I | 62 15 | |
| " " C | 3 52 | |
| | | 65 67 |
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| Special work on scrap-books | 10 00 | |
| Petty office expenses | 7 72 | |
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| Rent of office, $108; janitor, $100 | 208 00 | |
| | | 1,040 49 |
| Salary of Assistant Secretary | 600 00 | |
| " Permanent Secretary | 1,250 00 | |
| | | 1,850 00 |
| Grant to Professor Jastrow | 100 00 | |
| " " Hart | 100 00 | |
| | | 200 00 |
| Carried to Research Fund, life member commutation | 100 00 | |
| Carried to Research Fund, 1891, but not credited in account | 250 00 | |
| | | 350 00 |
| Balance to new account | | 1,875 46 |
| | | $8,433 37 |

certify that the same is correctly cast and properly vouched.

WILLIAM A. ROGERS, *Auditor.*

# INDEX.

# PROCEEDINGS

OF

# THE AMERICAN ASSOCIATION

FOR THE

# ADVANCEMENT OF SCIENCE

FOR THE FORTY-SECOND MEETING

HELD AT

MADISON, WISCONSIN,

AUGUST, 1893.



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